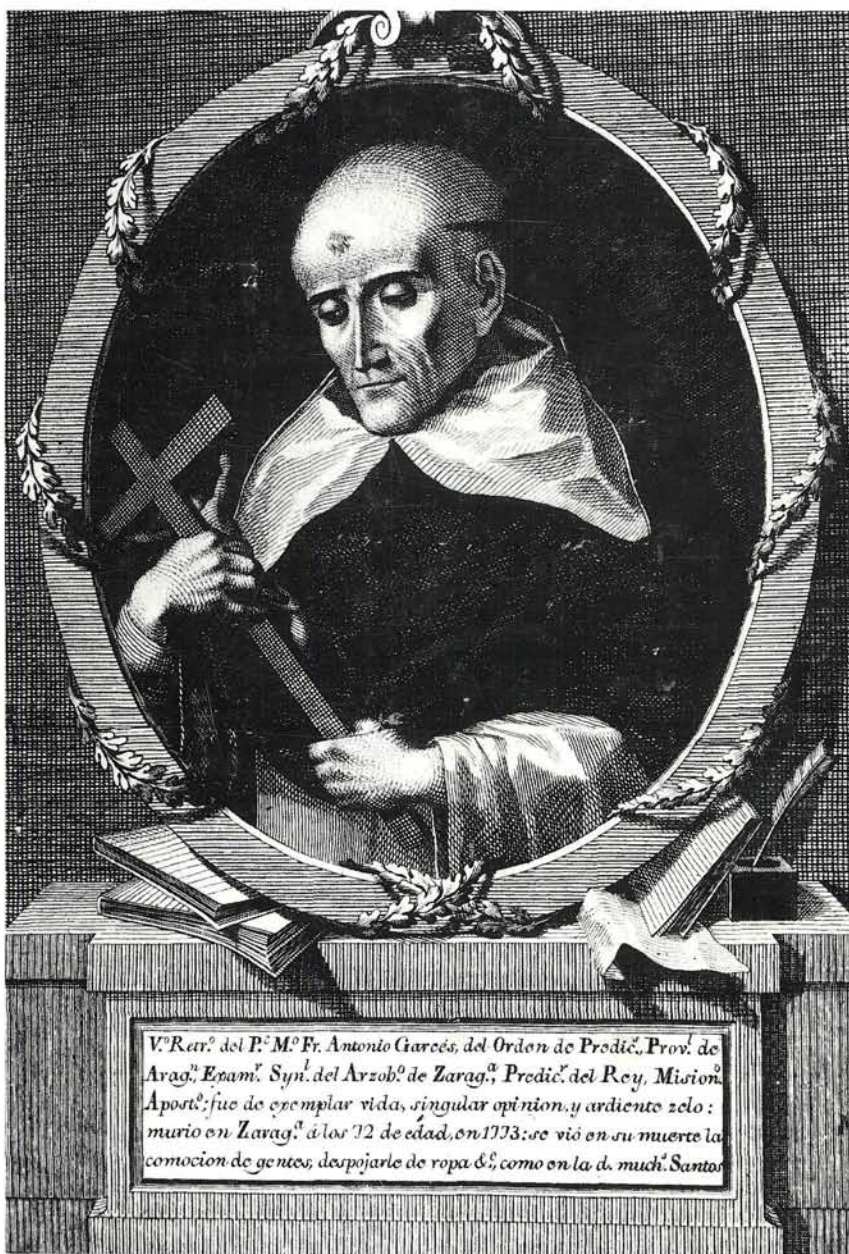


VOLUME 90 • NUMBER 4 • OCTOBER 1985

The American Historical Review

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The *American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 (202-544-2422) and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in 914 Atwater, Indiana University, Bloomington, Indiana 47405 (812-335-7609).

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes of \$40,000 and above, \$60.00 annually; \$30,000–\$39,999, \$55.00; \$20,000–\$29,999, \$47.00; \$15,000–\$19,999, \$40.00; \$10,000–\$14,999, \$30.00; below \$10,000, students, and joint memberships \$20.00; associate (nonhistorian) \$30.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 90: Class I, *American Historical Review* only, United States \$43.00, foreign \$47.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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Emotionology: Clarifying the History of Emotions and Emotional Standards

PETER N. STEARNS with CAROL Z. STEARNS

Emotionology: the attitudes or standards that a society, or a definable group within a society, maintains toward basic emotions and their appropriate expression; ways that institutions reflect and encourage these attitudes in human conduct, e.g., courtship practices as expressing the valuation of affect in marriage, or personnel workshops as reflecting the valuation of anger in job relationships.

Emotion: a complex set of interactions among subjective and objective factors, mediated through neural and/or hormonal systems, which gives rise to feelings (affective experiences as of pleasure or displeasure) and also general cognitive processes toward appraising the experience; emotions in this sense lead to physiological adjustments to the conditions that aroused response, and often to expressive and adaptive behavior.¹

HISTORIANS ARE PROBABLY JUSTIFIED in claiming that they are less prone than their colleagues in the social sciences to use jargon and invent new terms that often befuddle the uninitiated. And yet we have long been accustomed to use without second thought terms that were deliberately invented to identify for formal study clusters of related phenomena. "Sociology" itself had such an origin. Furthermore, few historians hesitate any longer to employ some of the terms sociologists have invented, such as "nuclear family," "extended family." What were once neologisms are now standard English and for the very good reason that they convey useful meanings that have heightened the precision with which we can express ourselves. The authors of this essay propose "emotionology" as a useful term with which to distinguish the collective emotional standards of a society from the emotional experiences of individuals and groups. Use of the term as defined above will focus our attention on the social factors that determine and delimit, either implicitly or explicitly, the manner in which emotions are expressed. Such a study will, we hope, illuminate how and why social agencies and institutions either promote or prohibit some kinds of emotions, while remaining neutral or indifferent to others.

As a subject, emotionology ought to fascinate twentieth-century Americans, who live in a society particularly conscious (or, at least, particularly interested in

¹ Paul R. Kleinginna, Jr., and Anne M. Kleinginna, "A Categorized List of Emotion Definitions, with Suggestions for a Consensual Definition," *Motivation and Emotion*, 5 (1981): 345-79, esp. 354-59.

proclaiming its consciousness) of emotional expressions and restraints. Historical work in this area is partly an effort at self-understanding, possibly because of our increased awareness of emotions as a facet of human experience. But the dependence of historical interest on contemporary issues is familiar, defensible, and not worth belaboring. The point is that all societies have emotional standards, even if they are sometimes largely unspoken or undebated, and societies differ, often significantly, in these standards. Anthropologists have long known and studied this phenomenon.² Historians are increasingly aware of it, as we realize that the emotional standards of societies change in time rather than merely differ, constantly, across space. Changes in emotional standards can in turn reveal much about other aspects of social change and may even contribute to such change.

But historians, and some anthropologists, who have properly grasped emotionology as a cultural variable, whose distinctions may affect other features of a society or group, have often confused thinking about emotion with the experience of emotion. For example, in a very interesting study, revealingly entitled *Never in Anger*, anthropologist Jean Briggs described how the Utku Eskimo tribe disapproves of anger and seeks to repress it through socialization. So far so good, for what is involved here is a distinctive value system and the set of informal institutions meant to express that system. But the study does not prove that the Utku are a people without anger. They regularly sulk when challenged (anger turned inward?) and, when even slightly provoked, routinely beat and otherwise abuse dogs and other household animals. These actions are not included in the tribe's proscriptions against anger because of the particular emotionology involved, but an observer may be pardoned for questioning whether the Utku are anger free.³

This point could seem fussy, were it not that some historians, in exploring the new field of the history of emotion, have exposed themselves to criticism by ignoring the distinctions between emotion and emotionology. They have written provocative histories about aspects of emotionology but have said far less about emotion than they have claimed. Their vulnerability is needless, or at least avoidable, but the remedy depends on a terminological distinction for which, without the neologism, we lack the vocabulary.

The study of emotion may become one of the hot new topics in social history, a field already known for expanding the range of investigation. The challenge, in sociohistorical terms, is obvious: if not only family structures, protest, and leisure pursuits but also aspects of emotional experience are legitimate subjects for historical inquiry, the social historian can go far toward demonstrating his claim that the study of history is necessary for understanding the context of ordinary

² In particular, anthropologists have long grappled with the problem of the influence of culture on emotional experience and expression; we are urging a similar, and more explicit, effort on the part of historians. See, for example, Ruth Benedict, *Patterns of Culture* (New York, 1961); John W. M. Whiting and Irwin L. Child, *Child Training and Personality* (New Haven, 1953); and Joel R. Davitz, *The Language of Emotion* (New York, 1969). Recent sociological inquiry has shown a revival of interest in cultural norms, which one author has termed "feeling rules." See Arlie Russell Hochschild, *The Managed Heart: Commercialization of Human Feeling* (Berkeley and Los Angeles, 1983).

³ Briggs, *Never in Anger: Portrait of an Eskimo Family* (Cambridge, Mass., 1970).

human life. One conclusion thus far advanced by historians in this field—that the emotional context of life in Western society has changed significantly in recent centuries—underscores the relevance of historical inquiry. We can use history to help understand our own collective emotional past, a heady prospect in a society encouraged to daily emotional temperature-taking. We may find that, until recent centuries, Western society viewed emotional expectations we take for granted “as a kind of mental illness” (a provocative interpretation that has been applied to the rise of romantic love in the eighteenth century).⁴ For if fundamental shifts in emotional standards have occurred, not only in our own recent past but also in different historical cases, we may seek clues in such shifts to other historical changes. To date, understandably enough, historians of emotion (the term emotional historians cannot be fairly suggested) in Western society have concentrated on the link between emotion and other changes in the family. But potentially the linkage can be expanded to include other activities, such as social protest or sports preferences, where emotion may be involved as more than a minor by-product.⁵

Historical study of emotions has the added attraction of linking social history to social and psychological theories that have largely been ignored in favor of more conventional sociological models of stratification and mobility. The history of emotion may help historians overcome, at least in part, a key weakness of psychohistory—the failure to deal persuasively with groups in the past.⁶ Further, social historians may be able to do more than simply use familiar psychological theory, which, because of the diversity of theories available and their tendency to be culture-bound, is a debatable practice, though a practice already evident in several relevant studies of emotional change over time.⁷ Historians may contribute to theory—that is, to an understanding of the dynamics of emotional behavior—in areas where psychologists have been hampered by their problems in dealing with change. This prospect, recently suggested in a general way by Theodore Zeldin,⁸ has been partly confirmed by explorations in American history of changes in the valuation of anger, an emotion not only viewed differently over time but also largely ignored (in favor of concentration on aggressive behavior) by contemporary psychologists, in part because of their own peculiarly modern biases concerning anger.⁹ The prospect of new interdisciplinary understanding, possibly one that enhances the historian’s contribution, is no small attraction of the extension of social history into emotional as well as rational aspects of past

⁴ Randolph Trumbach, “Europe and Its Families: A Review Essay of Lawrence Stone, *The Family, Sex, and Marriage in England, 1500–1800*,” in *Journal of Social History* [hereafter, *JSH*], 13 (1979): 136.

⁵ Abram de Swaan, “The Politics of Agoraphobia: On Changes in Emotional and Relational Management,” *Theory and Society*, 10 (1981): 359–85.

⁶ Richard L. Schoenwald, review of Robert J. Brugger, ed., *Our Selves/Our Past: Psychological Approaches to American History*, in *Social Science History*, 7 (1983): 345–47.

⁷ Lawrence Stone, *The Family, Sex, and Marriage in England, 1500–1800* (New York, 1977); and Christopher Lasch, *Haven in a Heartless World: The Family Besieged* (New York, 1977).

⁸ Zeldin, “Personal History and the History of the Emotions,” *JSH*, 15 (1982): 339–48.

⁹ Arnold H. Buss and Ann Burkee, “An Inventory for Assessing Different Kinds of Hostility,” *Journal of Consulting Psychology*, 21 (1957): 343–44; and Albert Rothenberg, “On Anger,” *American Journal of Psychiatry*, 128 (1971): 454–60.

mentalities. Finally, within the discipline of history, attention to emotional standards provides an opportunity for new contacts between social historians and their colleagues in cultural, religious, and intellectual history and may be a bridge between newer sociohistorical findings and more familiar themes, such as the rise of romanticism or the surge of marital piety in late medieval Catholicism. These contacts require caution (social historians must not neglect earlier discoveries about the limits of intellectual history in exploring popular culture and behavior), but, if properly handled, they may enrich the study of past emotional standards. Indeed, one function of the definition of emotionology is to improve the intersection of various historical approaches as they relate to change in, and the impact of change on, emotional values.

EARLY WORKS IN SOCIAL HISTORY, particularly on premodern European mentalities, reflect considerable interest in the emotional component of human experience. Lucien Febvre called for a "historical psychology" that would "give up psychological anachronism" and "establish a detailed inventory of the mental equipment of the time." His call was heard by a number of historians who, while rarely focusing on discrete emotions, tried to explain rituals, beliefs, and institutions in terms of the emotional climate of medieval and early modern Europe. Their interests were not, in the main, adopted by American social historians, who initially preferred to dissect measurable components of emotional outbursts such as witchcraft or avoid emotion-laden periods or incidents altogether. A key distraction was the tendency of American psychohistorians to assume a constant emotional apparatus, usually along Freudian lines, in preference to Febvre's more difficult (but more realistic) concept of a psychology itself subject to change over time.¹⁰

In fact, modern social history was born in the United States and Britain with a rather rationalist bent, not only in assumptions about scientific methodology but also in a tendency to claim sweet reason in the popular attitudes of the past. One of the first contentions of crowd historians, for example, was that rioters carefully selected their methods and goals—logical choices that can be easily grasped by historians once the rioters' basic assumptions are understood. The historical study of protest, indeed, remains dominated by the claim to rationality, to the extent that some authorities argue that emotion enters their subjects not at all. This is more than a refutation of the conservative, mad mob assumptions of Gustav Le Bon, the rationalists' first target. Charles Tilly has continued to see emotion as an irrelevant

¹⁰ Febvre, *A New Kind of History*, ed. Peter Burke and trans. F. Folca (New York, 1973), 9. On the implementation of Febvre's idea, principally by French historians, including Febvre himself, Georges Duby, and others, see Stuart Clark, "French Historians and Early Modern Popular Culture," *Past and Present*, 100 (1983): 62–99; Alphonse Dupront, "Problèmes et méthodes d'une histoire de la psychologie collective," *Annales-Économies, Sociétés, Civilisations*, 16 (1961): 3–11; Duby, "Histoire des mentalités," in C. Samaran, ed., *L'histoire et ses méthodes* (Paris, 1961), 937–66; and Jacques Le Goff, "Les mentalités: Une histoire ambiguë," in Jacques Le Goff and P. Nora, eds., *Faire de l'histoire*, 3 (Paris, 1974): 76–94.

by-product of protest, whose contours are firmly determined by organizational potential and rational crowd goals.¹¹

Other early sociohistorical works focused on areas where rational motivations could be assumed. Mobility studies, for example, tested responses to an objective social hierarchy; those who did not climb the ladder were presumed blocked by clear impediments, such as discrimination, or, in more subtle renderings, were seen following the rationality of a distinctive value system.

But a number of social historians, pursuing other aspects of the history of the common people, have not eschewed emotion as a component of the past they seek. It is almost impossible to examine the evolution of the family without considering emotional elements, although the effort has been made.¹² Without a parallel examination of emotional ties, structural studies of the links among kin, including extended kin, risk superficiality. The technical nuclearity of many premodern West European households is probably less significant, despite the attention lavished on its discovery, than the emotional ties that existed between elderly parents and adult children, unless, of course, those ties were random. Built around anger-producing child-rearing patterns and later conflicts over property control, the pattern of animosity within the premodern family, which several historians have uncovered, illuminates more of the content of premodern family life than does specification of household composition. And this emotional pattern proved susceptible to change, making it very much the historian's business. Greater affection between adult generations became visible when property relations became less tense, as in seventeenth-century New England, and a further increase in affection toward the elderly in the family has been hypothesized in studies of more recent American history. Structural changes may indeed pass almost unnoticed if they do not seem to disrupt—or if they abet—emotional patterns that researchers find familiar. This may be one explanation for the lack of adverse popular reaction to the growing residential discreteness of the elderly in mid-twentieth-century America, where bonds of affection remained unchanged, given frequent contacts between separate households, and possibly were promoted by the reduction of conflict between in-laws and kin.¹³

Demographic studies have also included analyses of emotional contexts, again almost inevitably. The most sensitive demographic historians have insisted that some change in family motivations must accompany major demographic changes, such as a reduction in birth rates; although certain relevant motivations involve rational economic calculations, others may involve emotions, such as the affection

¹¹ George F. E. Rudé, *The Crowd in History, 1730–1848* (New York, 1964); Tilly et al., *The Rebellious Century, 1830–1930* (Cambridge, Mass., 1975); and Tilly, *From Mobilization to Revolution* (Reading, Mass., 1978).

¹² Joan Wallach Scott, "History of the Family as an Affective Unit," *Social History*, 4 (1979): 509–16.

¹³ Peter Laslett, *The World We Have Lost* (New York, 1965); Philip J. Greven, Jr., *Four Generations: Population, Land, and Family in Colonial Andover, Massachusetts* (Ithaca, N.Y., 1970); David Hackett Fischer, *Growing Old in America* (New York, 1977); Daniel Scott Smith, "Accounting for Change in the Families of the Elderly in the United States, 1900 to the Present," paper presented at the Conference on the Elderly in a Bureaucratic Society, Case Western Reserve University, Cleveland, Ohio, April 1983; and Suzanne Pacaud and M. D. Lahalle, *Attitudes, comportements, opinions des personnes âgées dans le cadre de la famille moderne* (Paris, 1969).

extended toward and expected from children.¹⁴ Regardless of motivation, demographic shifts may themselves cause emotional change. That a reduction in infant death rates accompanies (whether as cause or effect) an increase in the emotional investment in children has become a historical commonplace. Other effects of demographic change on emotion have yet to be fully explored, including changes in the impact of grief as the incidence and manner of death have altered. But demography has already contributed significantly to the consideration of emotional standards within the context of family history.¹⁵

Most recently a variety of studies, which treat emotion as a legitimate focus of historical investigation, have directly confronted the emotional elements in family history. Historians working on France, Britain, Germany, and colonial America have uncovered a pronounced increase in familial affection in the late seventeenth and eighteenth centuries, which contrasts with the emotional tone seemingly characteristic of families in earlier centuries. John Demos noted an effort in seventeenth-century Plymouth to keep families free from the angry bickering more readily tolerated among neighbors; this effort to control anger was accompanied by the encouragement of conjugal love.¹⁶ Until somewhat later European families tolerated frequent outbursts of anger—or, at least, concentrated their efforts on avoiding behavioral excesses prompted by anger rather than recommending explicit emotional appraisals. Considerable ambiguities about the humanity of young children and concerted efforts at will-breaking fueled familial anger. These child-rearing methods produced behavioral restraints—in that children rarely expressed anger at parents until the latter grew old—but encouraged anger when other outlets were available, including the will-breaking administered to one's own children.¹⁷ Other accounts stress the cold emotional quality of premodern families more than the latent anger within them. The picture of emotional coolness is particularly marked in Edward Shorter's description of family life, for Shorter seems convinced that pronounced contrasts existed between the premodern and modern family, almost to the point that he describes distinct species. The same theme appears in some feminists' efforts to prove that maternal love is an entirely modern emotion and a snare imposed on women by society.¹⁸ But a similar vision emerges in other, less dramatic accounts. Lawrence Stone, for example, compared premodern familial emotion to the level of affect one would expect in a bird's nest.

¹⁴ Maris Vinovskis, "From Household Size to the Life Course: Some Observations on Recent Trends in Family History," *American Behavioral Scientist*, 21 (1977): 263–88.

¹⁵ Robert V. Wells, "Family Size and Fertility Control in Eighteenth-Century America," *Population Studies*, 25 (1971): 73–82.

¹⁶ Demos, *A Little Commonwealth: Family Life in Plymouth Colony* (New York, 1970), chap. 9. Also see Lloyd DeMause, ed., *The History of Childhood* (New York, 1974).

¹⁷ David Hunt, *Parents and Children in History: The Psychology of Family Life in Early Modern France* (New York, 1970); and Jean-Louis Flandrin, *Families in Former Times: Kinship, Household, and Sexuality*, trans. Richard Southern (Cambridge, 1979).

¹⁸ Shorter, *The Making of the Modern Family* (New York, 1975); Stone, *Family, Sex, and Marriage*; and Elisabeth Badinter, *L'amour en plus: Histoire de l'amour maternel (17^e–20^e siècles)* (Paris, 1980).

By the eighteenth century, even in Europe, this emotional pattern began to break up. Historians have disagreed vigorously over which social class pioneered change: the new proletariat, the middle class, and the aristocracy all have advocates.¹⁹ But they have agreed on the basic direction of change. Children began to be treated with greater and more intense affection, particularly, of course, where birth and infant mortality rates dropped.²⁰ Romantic love began to influence courtship and marital expectations (providing in some cases a novel motivation for the dissolution of marriage).²¹ On the eve of its decline as an economic unit, the family took on important new emotional functions, including the strengthening of affectionate relationships among family members. In a parallel development, attitudes toward animals began to shift toward more affection and away from the view that animals were a legitimate target for anger (the common pattern in most agricultural and hunting societies).²² These emotional shifts have been ascribed to various causes. Most scholars point to the role of religion. Protestantism urged greater attention to love between spouses, and variants of Protestantism, such as moderate evangelicalism in eighteenth-century America, carried the emphasis on familial affection and anger control even further. According to Jean-Louis Flandrin, Jansenism promoted similar religious motivations in France. The Enlightenment, by prompting greater egalitarianism within the family (and more humane attitudes toward animals) and by encouraging an individualism that sought emotional satisfaction, furthered the new emotional trends as well. Less defined but worth attention are economic factors. The rise of market relationships promoted individualism and the need to establish bonds outside the family. At the same time market relationships undermined more traditional emotional ties among male friends, leading men to turn to the family as an emotional haven safe from the competitive jostling that began to characterize male relationships.²³ The need to deal with and depend on strangers in a market economy also encouraged the control of anger, not only within families but also within neighborhoods.

The most intensive work on emotional change has focused on the decades preceding industrialization, which are a watershed in the history of emotion. Historians have been more tentative in analyzing later developments. At the end

¹⁹ Randolph Trumbach, *The Rise of the Egalitarian Family: Aristocratic Kinship and Domestic Relations in Eighteenth-Century England* (New York, 1978); J. H. Plumb, "The New World of Children in Eighteenth-Century England," *Past and Present*, 67 (1975): 64–95; Stone, *Family, Sex, and Marriage*; and Shorter, *Making of the Modern Family*. Also see, for the special issue on the history of love, *JSH*, 15, no. 3 (1982). Inevitably, the new findings about eighteenth-century emotional change are not, in fact, entirely new. Some time ago Norbert Elias discussed the emotional vagaries of premodern European society, noting the frequency of flashes of temper. His assessment of the rise of more genteel manners, though focused primarily on habits of politeness in dining and so forth, obviously relates to changing emotional expressions in social relationships, including a new effort to discipline temper and adopt a more consistently affectionate tone. See Elias, *The Civilizing Process: The History of Manners*, trans. Edmund Jephcott (New York, 1978), esp. 190–203.

²⁰ Wells, "Family Size and Fertility Control"; Philippe Ariès, *Centuries of Childhood: A Social History of Family Life* (New York, 1962); Plumb, "New World of Children"; and Philip J. Greven, Jr., *The Protestant Temperament: Patterns of Child-Rearing, Religious Experience, and the Self in Early America* (New York, 1977).

²¹ Trumbach, *Rise of the Egalitarian Family*.

²² Keith Thomas, *Man and the Natural World: A History of the Modern Sensibility* (New York, 1983).

²³ Edmund Leites, "The Duty to Desire: Love, Friendship, and Sexuality in Some Puritan Theories of Marriage," *JSH*, 15 (1982): 383–408; and Flandrin, *Families in Former Times*, 238–39.

of his study, Stone sketched the presumable rise of emotional repression in the Victorian era. Flandrin jumped the nineteenth century completely; he found the rise of romantic love in progress but incomplete by the end of the eighteenth century but neatly resumed, indeed completed and fulfilled, in the twentieth. And there is, generally, an assumption that the emotional standards first developed in the seventeenth century do describe a durable emotional framework for modern life, even if Victorianism or the separation of male work from home (leading to new emotional differentiation, in ideal types, between men and women) temporarily complicated realization of this development. Several recent studies of nineteenth-century courtship indeed reveal that ideals of romantic love remained important.²⁴ Studies of nineteenth-century child rearing, less surprisingly, show that recommendations for parental treatment of children continued to emphasize mutually loving relationships.²⁵ Other twentieth-century historians, following a new trend of looking nostalgically on the nineteenth-century middle-class family as yet another world we have lost, similarly assume a loving emotional intensity.²⁶ Questions remain about the impact of Victorianism on emotions, about emotional life among the lower classes, and, now, about the relationship of contemporary familial emotion to eighteenth- and nineteenth-century standards. But the idea of a new period in Western emotional history, corresponding to what we call modern in political or economic history, seems well established.

And this idea is an important contribution to historical study. Historians can now point to major emotional shifts over time, just as anthropologists have pointed to different emotional experiences—and expressions, including even some of the facial expressions used to denote basic emotions—across cultures. Henceforth, emotional changes must be considered, along with other shifts in mentality and behavior, as part of the historian's attempt to convey and explain sociohistorical change. Whether emotional changes *cause* other fundamental changes—as shifts in the emotions involved in child rearing may have done in modern history—or more commonly *reflect* other basic factors, like ideology or economic relationships, must still be discussed. But that emotional change needs to be woven into the historical fabric seems unquestionable. One consequence would be a valuable contribution to psychohistory. The assumption of psychological constants, which allows broad application of presumed contemporary psychological dynamics to past personalities, needs rethinking in light of the discovery of emotional shifts. No longer can we assume without proof, as in a recent study of nineteenth-century grief, that people in the past shared our emotional experience, that we can use contemporary psychology to elucidate past behavior, or that we can use past data,

²⁴ Ellen K. Rothman, "Sex and Self-Control: Middle-Class Courtship in America, 1770–1870," *JSH*, 15 (1982): 409–26; and Patricia Branca, *Silent Sisterhood: Middle-Class Women in the Victorian Home* (London, 1975), 122–29. But Zeldin's portrait of the French bourgeois family complicates this pattern by introducing an important national variant; Zeldin, *France, 1848–1945*, volume 1: *Ambition, Love, and Politics* (Oxford, 1973).

²⁵ Bernard Wishy, *The Child and the Republic* (Philadelphia, 1968); and R. Gordon Kelly, *Mother Was a Lady: Self and Society in Selected American Children's Periodicals, 1865–1890* (Westport, Conn., 1974).

²⁶ Lasch, *Haven in a Heartless World*.

without careful analysis, to bolster contemporary psychological theory.²⁷ Linkages may be possible, but they require evaluation and evidence. They cannot be taken for granted as premises for research.

Yet, for all the legitimate excitement that the new findings have roused in historians prepared to examine emotional factors in history, the first wave of research has left not only important work to be done but also a nagging sense of dissatisfaction. It is this dissatisfaction that must be addressed, through use of a new terminology, before additional research is undertaken.

SEVERAL CRITICS HAVE BEEN TROUBLED by the sharp contrast drawn between a cold premodern and a loving modern society. Have we really shifted from an aversion to infants as greedy, animal-like creatures to child-rearing attitudes based on affection? The idea correctly suggests that some change has occurred and serves as a useful antidote to the time-bound assumption of an eternal kind of maternal affection. But it is probably too stark. Obviously, some allowance must be made for individual variation from the norm. We know that affection for babies is not uniform in our own society. And we might correspondingly assume some deviations from the attitudes of hostility thought to exist in preindustrial times. Beyond the issue of exceptions, however, which some generalizations gloss over, scientific findings need to be integrated, some of which demonstrate that maternalism, though definitely not a historical constant, does have biological components that probably made pre-eighteenth-century practice less distinctive than pre-eighteenth-century rhetoric implies. Thus, glandular changes associated with breast-feeding, which lead to maternal bonding, are not recent developments. To be sure, large numbers of mothers did not breast-feed in the seventeenth century (particularly in parts of Western Europe), and they therefore did not experience this natural bonding. But the majority did, and their experience must be taken into account.²⁸

Critics of an early modern "romantic revolution" in courtship and marriage have focused more strictly on individual exceptions to the concept of preromantic coldness, for biological constants are murkier in this area. Cases of dramatic passion for a spouse have been found before the shift to frankly romantic marital criteria, and, although these instances did involve a defiance of accepted norms, their existence must qualify generalizations about emotional style.²⁹ Even if

²⁷ Paul C. Rosenblatt, *Bitter, Bitter Tears: Nineteenth-Century Diarists and Twentieth-Century Grief Theories* (Minneapolis, 1983).

²⁸ What we suggest here is a balance—its exact dimensions to be determined by careful analysis of data—between the overly categorical claims of the sociobiologists for eternal biological characteristics and the overly sharp claims made recently by some historians that virtual reversals in emotional experience have occurred. Edward O. Wilson, *On Human Nature* (Cambridge, Mass., 1978), and *Sociobiology: The New Synthesis* (Cambridge, Mass., 1975). Our suggestion fits the effort at a humanistic reconciliation of both positions in Kenneth Bock's *Human Nature and History: A Response to Sociobiology* (New York, 1980), 184–85. For a recent historical contribution on emotional constants, see Linda A. Pollock, *Forgotten Children: Parent-Child Relations from 1500 to 1900* (Cambridge, 1984).

²⁹ Barbara J. Harris, "Marriage Sixteenth-Century Style: Elizabeth Stafford and the Third Duke of Norfolk," *JSH*, 15 (1982): 371–82.

preromantic emotional standards for courtship are accepted as having been fairly uniform, it is clearly imprudent to assume that romantic love might not have sometimes affected mate choice or developed during marriage, though this kind of affection was not recommended before the seventeenth century. Aside from expectations that some husbands and wives, despite a different emotional context, developed strong affectionate ties after years of cohabitation and shared work, the recent discovery of a high incidence of love-affair complaints in seventeenth-century (that is, preromantic) medical practice in England casts doubt on the uniform coldness of marital and premarital ties.³⁰

These qualifications touch the contemporary pole of the comparison as well, where documentation is more readily available. As suggested earlier, several students of the eighteenth-century upsurge in romantic love have written as though the trends then launched revolutionized marital relations; Flandrin, most explicitly, suggested that fruition of the love ideal came about in the twentieth century. But romantic love in twentieth-century marriage is hardly uniform. A poll taken in France in the 1960s revealed that slightly more than half of all boys in secondary school did not expect or desire a romantic marriage (though over 80 percent of the girls did, a disparity that suggested future marital conflict).³¹ Studies of working-class marriage, with their emphasis on the mother-daughter bond as the primary affectionate tie even for young married women, raise similar doubts about the triumph of the romantic love ideal.³² As in the case of child rearing, reality seems to be more complex, change less dramatic, than some of the pioneers in the history of love have indicated.

Other claims made by historians in this field are suspect from another angle. Several have written of a decline in anger concomitant with the eighteenth-century increase in romantic emphasis: "The quality of attachment dramatically improved in the eighteenth century, and, among other things, decreased the level of aggressive violence."³³ At one level—the emotionological—this hypothesis is surely correct: the authors of family and child-rearing manuals urged both emotional goals simultaneously, from the eighteenth to the mid-nineteenth century. But did anger really decline as a result? And has the goal of anger control remained tied in more recent history to recommendations of familial affection? Certainly child-rearing manuals by the late nineteenth century included more positive roles for anger, though not within the family, than their mid-Victorian predecessors had done, yet their emphasis on the need for affection remained unabated. And it is surely possible that growing emotional intensity brought both anger and love in its wake, a notion found in popular culture and in Stone's brief description of the tolerance toward unrestrained displays of emotion—public fighting as much as

³⁰ Michael MacDonald, *Mystical Bedlam: Madness, Anxiety, and Healing in Seventeenth-Century England* (New York, 1981).

³¹ Zeldin, *France, 1848–1945*, 1: 128; and Evelyne Sullerot, *Women, Society, and Change*, trans. M. Archer (New York, 1971).

³² Mirra Komarovsky, *Blue-Collar Marriage* (New York, 1964); and Peter Willmott and Michael Young, *Family and Kinship in East London* (New York, 1957).

³³ Trumbach, "Europe and Its Families," 139.

public displays of affection—in the decades before the Victorian era.³⁴ Again, the history of emotion seems to be more complex than some of the most striking accounts have yet allowed. Love may not have gained ground so uniformly, because of complications at both ends of the chronological spectrum, and its relationship with other emotions may be more subtle than some observers have believed. We have, in fact, only begun to examine past emotional experience, and the inquiry promises to be arduous. Loving behavior by parents and, still more obviously, by spouses has not increased at the same pace as recommendations of both, and these behavioral patterns did not depart from such a uniformly cold background as has been imagined. Actual patterns of anger cannot be assumed from recommendations of control.

Many historians of emotion have been trapped, ironically, by some of their own cultural assumptions. We live in a society that places an unusually high value on romantic love. It is proper and illuminating to seek the origins of this attitude. But we must also beware of how our own strong assumptions can obscure our view of the past. Surely an idealization of romantic love helped prompt historians of the working class, like E. P. Thompson, to object to Stone's assertion that romantic ideals began nearer the top of society.³⁵ (Interestingly, no one has objected to recent findings that nineteenth-century workers were less sexually repressed than the bourgeoisie, now that sexual freedom is seen as desirable.) For why argue that workers built romantic love into familial expectations as early as the middle class did, when there is evidence that even now familial affection differs between the two groups, with workers focusing less on husband-wife ties and more on affectionate links in the extended family? Of course, there are major empirical questions involved, concerning present as well as past class differentiations, but it is not inherently demeaning to suggest some differences in emotional style save as historians themselves are wedded to modern middle-class emotionology. As we have suggested, romantic ideals have sometimes prevented observers, historians and others, from recognizing emotional variations, even in studies of the twentieth-century middle class. Certainly the assessment of premodern family styles is complicated by a time-bound reaction to practices that in our culture denote a lack of proper feeling. Affection in premodern society may not have been as different as vocabulary and child-rearing and courtship behaviors suggest to the modern researcher. Expressions of anger may have been more compatible with love for children than our modern values allow.³⁶ Or affection may have been more dispersed—spread among friends of the same sex, for example—but no less real

³⁴ See Stone, *Family, Sex, and Marriage*, 451. Here, and in much of what follows, the discussion of anger is drawn from a forthcoming book on anger in American history, by Carol Z. Stearns and Peter N. Stearns, entitled *Anger: The Struggle for Emotional Control in America's History* (Chicago, 1986).

³⁵ Edward P. Thompson, review of Lawrence Stone's *The Family, Sex, and Marriage in England, 1500–1800*, in *New Society*, 8 (1977): 499–501.

³⁶ Herman W. Roodenburg, "The Autobiography of Isabella de Moerloose: Sex, Childrearing, and Popular Belief in Seventeenth-Century Holland," *JSH*, 18 (1985): 517–40. This study shows the clear presence of maternal love in a premodern family, but it was combined with unembarrassed (if to the child disconcerting) flashes of maternal rage that add up to an emotional package different from what contemporary emotionology would lead us to expect.

for being hard to identify by modern observers trained to associate intense affection with marriage or its functional equivalent in extramarital cohabitation (the pattern that once was called concubinage before it was taken up by young middle-class couples). The facts must be faced: the finding most obviously desired from a history of emotion—some plausible chart of the rates of love and anger or the experience of sexuality, joy, and grief—is unlikely to emerge with any precision; it definitely has not emerged to date, despite historians' efforts to postulate trends.

The concept of emotionology is necessary, quite simply, to distinguish between professed values and emotional experience. It also aids researchers seeking to identify contemporary emotional values that could bias any inquiry, historical or otherwise, that attempts to assess the emotional experience of others. As we realize that some of our emotional values—including findings hailed as scientific about the importance of love and of certain kinds of loving expressions in the socialization of children—are influenced, if not created, by the emotionology of our own times, we can more intelligently evaluate past indications of emotional values and assess the origins of our own assumptions.

The emotionology-emotion distinction presumes that in most cases the emotionological range is greater than the variations in emotional experience, from one society or period to the next. Shifts in affection toward infants have probably occurred in Western society over the past three centuries, but they have not been as great as the shifts in emotional standards regarding infants. We can maintain the earlier claim—that psychohistorians must take greater account of changes in emotional behavior over time, given the findings of alterations in behavior even in recent centuries—while admitting that certain psychological findings probably do describe human realities that may be immune to change. After all, we are animals with biological constraints; it is curious that many historians and social scientists have ignored biological factors in their studies of emotion. One of the challenges of research in this field is to sort out the durable (animal) from the transient (culturally caused), while juxtaposing both with prevailing emotional standards.

Most studies of the history of emotion, though not all, have dealt far more with emotionology than with emotional experience. Historians have even neglected one of the central lessons of social history, that formal ideas cannot be automatically equated with wider social values, much less with actual feelings, without a good bit of independent verification. This applies to some of the most facile claims, for example, that maternal love is an eighteenth-century invention or that a major shift in the American valuation of love has occurred in the later twentieth century, an interpretation based on reviewing a handful of highbrow movies.³⁷ The best work has incorporated aspects of emotionology from research in a variety of sources: formal writings on emotion, like those of Protestant theologians in the seventeenth century; more popularized literature, like the sermons directed at family behavior;

³⁷ Ann Swidler, "Love and Adulthood in American Culture," in Neil Smelser and Erik Erikson, eds., *Themes of Work and Love in Adulthood* (Cambridge, Mass., 1980), 120–50; and Badinter, *L'amour en plus*.

and resulting behavioral patterns, such as altered courtship practices or the abandonment of swaddling. But there has nevertheless been too much temptation to assert novel emotional experience, on the basis of admittedly novel emotionology, than the facts warrant. More precise historical aperçus may also have been exaggerated. Were families in Plymouth really exempt from anger, as Demos has asserted (or, to allow for human frailty even among new Americans, largely exempt)? Or should the first distinction be, simply, that the unusual values held up for family behavior—the emotionology—expressed the importance of controlling anger in contrast to prevailing European norms, which would have affected judgments of familial anger without necessarily describing emotional experience within families? Because modern emotionology has influenced the location of emotional expression—what can be shown in public and in private—the task of appraising emotion is particularly complex.³⁸

THE DISTINCTION BETWEEN EMOTIONOLOGY and emotional experience suggests a clearer research strategy than most historians have so far pursued. Inquiry into love, anger, jealousy, and fear³⁹ should begin with the emotionological context, which is more accessible than emotional experience and important in its own right. Emotionology includes behavior, such as courtship, that reflects and is meant to enforce social norms. In the second stage of inquiry historians should attempt to fathom emotional expressions across time, assuming a correspondence between these trends and those of emotionology but alert to the possibility of variance in direction as well as in degree. In some cases historians may be unable to investigate beyond emotionology; certainly most records of emotional expression, such as diaries, will be filtered by prevailing emotionology. Still, only a distinction between emotion and emotionology will produce the best possible evaluations of past emotional experience.

There is a third task, not an analysis of emotionology proper but related to it, which involves examining peoples' efforts to mediate between emotional standards and emotional experience. This subject may be accessible even when changes in emotions cannot be charted, and it plays a role in family history, where anguish over the presence or absence of certain emotions may change more than the emotions themselves. It is much clearer, for example, that guilt over familial anger has risen since the eighteenth century, as the result of new emotionology, than that the frequency and degree of anger have changed. Yet the presence of guilt is itself important, as part of family life and overall emotional experience. This third stage of inquiry can be readily pursued through diaries and other sources, which are likely to reveal reactions to emotion more than emotion itself. In this process emotionology remains partially discrete from a more general interest in popular

³⁸ George R. Bach and Peter Wyden, *The Intimate Enemy: How to Fight Fair in Love and Marriage* (New York, 1968), 17–34. Also see the chapter on family in Stearns and Stearns, *Anger*.

³⁹ Jean Delumeau, *La peur en Occident, XIV^e–XVIII^e siècles: Une cité assiégée* (Paris, 1978); Jacques LeBrun, *La peur* (Paris, 1979); and Madeleine Läk, *La peur qu'on a* (Paris, 1979).

mentalities. For emotionology interacts with emotional experience, even when it does not describe this experience exactly, and, therefore, can help reveal the relationship of the experience, appropriately filtered, to other behaviors and perceptions in the past.⁴⁰

In addition to anthropologists who have struggled with the relationship between culture and emotion, a few sociologists have worked on a framework appropriate for the differentiation between emotions and emotionology. William Goode, for example, wrote of an emotionological range between viewing love as a reprehensible aberration, as in the East Asian tradition, and viewing love with considerable approval, such that it is mildly shameful to marry without "being in love." But this range does not prevent deliberately loveless marriage in an age that approves the romantic extreme or vigorous love attachments and literary idealizations of love in an age of emotional repression.⁴¹

Historians themselves have a clear analogue to the necessary distinction between emotion and emotionology in recent findings on sex. Victorian culture is no longer seen as automatically defining Victorian sexual experience, and not only because of the class divisions that long exempted much of the working class from extensive contact with the culture. Both ideals and experience are significant. Sexual ideals influence practice, attitudes toward practice, and institutional arrangements, such as birth control or the treatment of sexual "deviants," but sexual experience, chartable through illegitimacy rates and to an extent through diaries, autobiographies, and other sources, also causes and reflects important social patterns. A full history of sex—to use the term generically—must include both aspects. Similarly, emotionology is itself important even when it does not define emotional expression. Admittedly, it may prove harder to trace emotion than it is to delineate sexual patterns, only because evidence for the latter may be quantifiable, but the quest is not impossible and can be facilitated by a recognition of the distinction between emotions and the criteria used by society to channel and evaluate them.

For example, during the late eighteenth century, the term "tantrum" first came into the language (its origins are unknown but perhaps derive from the Italian dance tarantella). The word was used disapprovingly in reference to angry behavior by adults. During the nineteenth century the term became widespread—though it entered dictionaries only after 1900—and acquired its current meaning, a rage by children or, in adults, a particularly childish fit. Here, clearly, is a significant shift in emotionology, denoting a new awareness of a particular kind

⁴⁰ Emotionology in this sense is more than a word-saving term that eliminates the necessity of repeating phrases like "ideas about fear" or "the popular culture of jealousy." When applied to religious experience, recreation, and popular art, it can be more precisely linked with discrete emotions than can other, more general terms. Excellent studies of premodern mentalities that undeniably describe some emotional content are thus somewhat different, pending a more grandiose synthesis, from studies of emotionology. For example, Johan Huizinga and Max Weber, in their classic works, described popularized intellectual moods, or mentalities, that may feed an emotionology, or derive from it, but they dealt with more purely cognitive or aesthetic reactions—reactions more fully linked to formal culture—than real emotionology entails. See Huizinga, *The Waning of the Middle Ages* (New York, 1955); and Weber, *The Protestant Ethic and the Spirit of Capitalism* (New York, 1977). On other studies that use emotion as part of mentalities, see the references in note 10, above.

⁴¹ Goode, "The Theoretical Importance of Love," *American Sociological Review*, 24 (1959): 38–47.

of anger and, through awareness, a new ability to disapprove of it or to try to prevent it through socialization. But surely the behavior described by the word was not new, as the simplest equation between emotionology and emotion might imply. Nor has the invention of the word, or socialization practices designed to modify the behavior, led to a demonstrable diminution of the phenomenon. A few permissive child-rearing experts did indeed claim in the late 1940s that tantrums were on the wane, but they offered no evidence, and their assumption may not be open to serious historical investigation. Clearly, a history of the perception of tantrums, though significant, is not likely to be identical to that of childish behavior.⁴²

The emotionology-emotion distinction bears on historical periodization. Emotionology changes, and its changes will normally have some impact on emotional experience or perceptions of it. But emotional experience will not only reflect emotionology juxtaposed to psychological constants but also may reveal the impact of developments such as, in the nineteenth century, the separation of work from home. The relationship between emotionology and emotion thus may not be invariable, which is one reason why so many historians have paused at the end of the eighteenth century without extending their investigations into a subsequent period in which emotionology showed substantial continuity, as in recommendations about love and anger, but without the same rapport with actual emotional life. An effort to define the changing links between emotionology and emotion, as each responds to a somewhat different dynamic, may provide the means to approach appropriate periodization in both areas.

Emotionology is also a vital concept in dealing with the complex impact of class and gender (and possibly age) on emotional experience. The dominant emotionology of a society may set norms for men and women (as in the nineteenth century) or for age groups (as in the preindustrial period, when frequent anger and greed were attributed to the elderly). These norms, though important in their effect on perceptions of proper behavior, do not necessarily describe emotional experience. We know that women have felt anger more frequently than modern emotionology, until recently, countenanced. Their behavior and emotional experience may have been influenced by emotionology, as when anger was concealed within family disputes or revealed in hysteria, but their emotions departed from emotionological standards. A similar distinction between values and reality has been demonstrated in contemporary American society by findings that show a boy's temperament much more predictive of adult personality than a girl's; girls' socialization remains more influenced by emotionological standards, which often yield in adult emotional behavior, as angry women bloom from docile girls.⁴³ In another important case, old people were not necessarily angrier or greedier in past periods when conventional emotionology pointed specifically to

⁴² Leon Kanner, *In Defense of Mothers* (New York, 1944), 37; Dorothy W. Baruch, *New Ways in Discipline: You and Your Child Today* (New York, 1949), 7, 45, 61; Dory Metcalf, *Bringing Up Children* (New York, 1947); and Marion J. Radke, *The Relation of Parental Authority to Children's Behavior and Attitudes* (Minneapolis, 1946), 11–12. Also see the chapters on child rearing in Stearns and Stearns, *Anger*.

⁴³ Jerome Kagan and H. A. Moss, *Birth to Maturity: A Study of Psychological Development* (New York, 1962).

these traits, for the emotionology reflected not just experience but also tensions over property that might change independently of any changes in emotional behavior. That emotional labels applied to the elderly began to change in the nineteenth century is important, but it does not prove corresponding emotional change.

With regard to class, emotionology facilitates understanding of the extent to which emotional criteria are purveyed, explicitly or not, by a dominant class culture. A number of the familiar nineteenth-century middle-class criticisms of working-class culture have an emotionological component; for example, workers were judged insufficiently affectionate to wives, children, and animals. Less familiar, but probably more significant, is a twentieth-century effort to extend new middle-class values concerning the control of anger to the judgment and treatment of the working class.⁴⁴ In this case the emotionology of a single class can have general social ramifications, without accurately describing the actual emotional experience, or even the emotionology, of other social groups. Analysis of emotionology can enrich an understanding of class efforts to create a dominant culture and can help sort out disputes over "trickle down" theories of emotional change, like those proposed in studies of eighteenth-century England. That the middle class or the aristocracy pioneered emotionological change on the eve of modern times is hardly surprising, but this does not rob lower classes of an emotional culture of their own (yet to be fully fathomed) and definitely requires an assessment of emotional differences among classes, if any, independent of the professed values of the dominant groups. Emotionology can indeed facilitate study of the emotional experience of lower classes by helping isolate an amazingly persistent middle-class bias, even in twentieth-century efforts to examine working-class emotional standards.⁴⁵

Analysis of emotionology does not, of course, automatically resolve problems of assessing class or gender experience. It does not guarantee the availability of sources needed to study emotion or even emotionological subcultures. It does, however, help guard against generalizations derived from inappropriate sources, while providing the kind of distinction between approved values and actual experience that has informed other studies in social history.

THE DISTINCTION BETWEEN EMOTIONOLOGY and emotion and the resulting stages of research raise two obvious questions: can the history of emotions be directly tapped? And, if it cannot with any definiteness, is emotionology worth attending to?

It is clear that the effort to deal with changes in emotional experience has barely begun, despite some striking claims by historians. Emotions almost surely retain

⁴⁴ Loren Baritz, *The Servants of Power: A History of the Use of Social Science in American Industry* (Middletown, Conn., 1960). Also see the chapter on work in Stearns and Stearns, *Anger*.

⁴⁵ Daniel R. Miller and Guy Swanson, *The Changing American Parent: A Study in the Detroit Area* (New York, 1964). For some questions about class stereotypes in sociological research on emotional styles, see M. R. Yarrow et al., *Child-Rearing: An Inquiry into Research Methods* (San Francisco, 1968).

some important biopsychological continuities. Changes are going to be slower and less concrete than historians, who like dramatic upheavals, would prefer. There may not be change in basic emotions at all. Yet emotion has cognitive components that are open to emotionological influences. It is likely—though not inevitable—that emotionological change will normally have some bearing on emotional experience. When romance is discouraged in courtship, for example, there will probably be less romance (although some) in marriage than when it is encouraged, if only because couples lack appropriate expectations or, in some cases, the vocabulary necessary to express them.⁴⁶ Even biological components of emotions may change, for example, when the incidence of breast-feeding shifts or the health (therefore the mood?) of the elderly improves. And if emotions change, even subtly, the possible ramifications on other aspects of behavior, including political, are great enough to command serious attention from historians in several fields.

Because distinctions between standards and emotions have not been sufficiently explicit, we do not yet know how much of the history of emotion we can uncover. After a period in which sharp emotional distinctions were drawn by historians, we seem now to be moving into a revisionist phase in which substantial continuity is stressed. Thus, recent examinations of diaries conclude that reactions to grief have not changed from the nineteenth to the twentieth century and that parental love for children was as lively in the sixteenth century as in the nineteenth.⁴⁷ Another interpretation urges that emotional variables play an important role in history, even influencing political behavior, without being subject to change over time. Philip Greven, though once arguing that emotional styles did shift toward a more loving approach to the child,⁴⁸ recently pointed to the importance of group variation within a time period, which he treated as more significant than an effort to establish change.⁴⁹ Greven saw three fundamental emotional styles present in American families by the late eighteenth century, which have not clearly shifted over time; at most, the balance among them may have altered but in a cyclical fashion more than in long-term changes in direction. This interpretation needs to be tested, of course, in other time periods; as a theoretical construct it has not been fully developed. But its assumptions are conceivable in principle; it correctly calls attention to subgroup variations in emotion that may display greater continuity than do the presumed standards of “society” at large. And Greven’s own work

⁴⁶ Recent research on emotions indicates that the cognitive framework within which one classifies an experience affects the emotion, even its autonomic manifestations. For a summary of this phenomenon, see Kleinginna and Kleinginna, “Categorized List of Emotion Definitions,” 350. In fact, some of the most promising recent work on the psychotherapy of depression stresses restructuring the patient’s cognitive style to teach him to think about his experiences more positively. This, in itself, has helped cure patients overwhelmed by depressive emotions. In view of the Freudian assumptions that underlie most American social sciences, it behooves us to be aware that thought may influence feeling as well as the reverse. See A. John Rush and Donna E. Giles, “Cognitive Therapy: Theory and Research,” in A. John Rush, ed., *Short-Term Psychotherapies for Depression* (New York, 1982), 143–81.

⁴⁷ Pollock, *Forgotten Children*; and Rosenblatt, *Bitter, Bitter Tears*. For more strictly emotionological evidence, see Steven Ozment, *When Fathers Ruled: Family Life in Reformation Europe* (Cambridge, Mass., 1983).

⁴⁸ Greven, ed., *Childrearing Concepts, 1628–1861* (Itasca, Ill., 1973).

⁴⁹ Greven, *Protestant Temperament*.

demonstrates that the search for key emotional groupings, in a single period, has important implications for a national history.

Efforts to get at emotions, particularly when introducing continuities or complexities not discernible at the strictly emotionological level, have relied heavily on evidence found in diaries. Whether this evidence is representative, and whether it is sufficiently free from emotionological filters, is questionable. Ideally, a history of emotion must use diaries and autobiographies in combination with other sources, including literary portrayals of emotion (though these may primarily reflect the dominant emotionology), other commentaries on emotional expressions in family, work, or leisure life, and, with due caution, behavioral indexes, such as the rates and tenor of protest. A combination of these kinds of sources suggests, for example, real change in the emotional experience of work in the twentieth-century United States, though the suggestion falls short of absolute proof and does not indicate what happens to emotions displaced from the job site.⁵⁰ Just as it is interesting that the first studies of emotions posited unduly sharp shifts in experience, based on emotionological change, so it is important to note that recent efforts to demonstrate continuities—maternal love is maternal love all along—hardly commend themselves by the variety of data they have employed. The jury on emotional change, it is fair to say, is still out.

If the task before us in dealing with emotional shifts is difficult, and inevitably somewhat tentative, it is particularly arduous in the study of emotions among the lower classes. Here, commentary by outsiders is almost always biased by emotionological standards. Even the relevant emotional values preached in working-class families—whether followed or not—are not always clear. But the difficulty of examining basic emotions should not, at least as yet, deter the attempt. Careful use of a multiplicity of sources, aided by the distinction between emotional values and emotional experience, may yield clearer signposts than we have to date. It is unwise and unnecessary to use difficulty as an excuse for neglect of a basic ingredient of human history. Nor should problems in studying emotion deter us from continued inquiry into emotionology at various social levels—a subject important in its own right.

In the first place, emotionology is not some simple if specialized artifact of intellectual history. It is clearly influenced by formal ideas, as the work on eighteenth-century love has amply demonstrated. But one challenge in the study of emotionology is to place intellectual aspects in a larger cultural context. Thus, a strictly intellectual history of romantic love logically begins with the tradition of courtly love,⁵¹ but it is most unlikely that a history of emotionology should do so. For the tradition of courtly love, though reechoed in some Renaissance literature, simply did not penetrate far enough into popular culture or into institutional arrangements to count as a genuine emotionology. Similarly, in our own century, an expert, sociological critique of romantic love as the basis for marriage clearly

⁵⁰ Baritz, *The Servants of Power*; and Hochschild, *Managed Heart*.

⁵¹ Herman R. Lantz, "Romantic Love in the Pre-Modern Period: A Social Commentary," *JSH*, 15 (1982): 349–70.

cannot be equated with emotionology, which has largely resisted the hostility of experts.⁵² We still manifest a widespread cultural assumption of love before marriage, and for the most part we have institutions to match. A serious shortcoming in Christopher Lasch's challenging comment on recent emotional and familial change is the assumption that a history of ideas about the family, including familial emotion, adds up to a widely shared culture.⁵³

In the study of emotionology, in other words, many of the dilemmas involved in bridging the gap between intellectual and social history, which occur and have been studied in other areas, are repeated. Therefore, although emotionology does help relate ideas and popular culture to a powerful theme in human life, it provides no simple equation. The emotionology of a society often responds to economic or demographic change, for example, in efforts to change the valuation of anger to correspond to corporate personnel requirements⁵⁴ or in the impact on the emotions of love and grief of changes in child mortality. The best statements of causation in the field thus far have intermingled religious and intellectual change with a sense of shifting organizational context. In this sense emotionology, relating so closely to basic human reactions (though, of course, not necessarily constituting these reactions), pulls together a number of strands of fundamental historical change, at least in certain periods in which key values are in flux.⁵⁵

More important still, emotionology has an impact on other areas of social behavior, apart from any effect it may have on emotions themselves. This is true in the evolution of the modern family, which serves as a center for emotional expression. The rise of divorce (along with other factors) thus mirrors expectations of love in marriage and sometimes low tolerance for the expression of anger in marriage—attitudes, in other words, that follow from a dominant emotionology. Divorce itself, in the twentieth century, is one of the (relatively few?) rituals where the expression of intense anger is readily tolerated. No historian could presume to explain the history of divorce solely by an examination of the impact of emotionology on the family, but none can avoid the topic. Emotionology concerning one set of family relationships may affect others. Thus, romantic love between couples may have followed, emotionologically, from an earlier change in child-rearing standards, as romantic love was essential to permit children to free themselves from what was assumed to be the power of the parental emotional

⁵² William L. Kolb, "Sociologically Established Norms and Democratic Values," *Social Forces*, 26 (1948): 451–56; and Ronald L. Howard, *A Social History of American Family Sociology, 1865–1940* (Westport, Conn., 1981).

⁵³ Lasch, *The Culture of Narcissism* (New York, 1979), and *Haven in a Heartless World*.

⁵⁴ Miller and Swanson, *Changing American Parent*; Susan Porter Benson, "The Clerking Sisterhood: Rationalization and the Work Culture of Saleswomen in American Department Stores, 1890–1960," in James Green, ed., *Workers' Struggles, Past and Present: A Radical American Reader* (Philadelphia, 1983), 101–07; and Hochschild, *Managed Heart*. On economic factors in eighteenth-century emotionological change, see Edward Shorter, "Illegitimacy, Sexual Revolution, and Social Change in Modern Europe," *Journal of Interdisciplinary History*, 2 (1971–72): 237–72; Benjamin Nelson, *The Idea of Usury* (Chicago, 1969); and Leites, "The Duty to Desire," 383–408.

⁵⁵ Nelson, *Idea of Usury*; and de Swaan, "Politics of Agoraphobia." Walter J. Ong has plausibly argued that rising literacy rates cause new emotional standards, though he offers no direct emotionological evidence. He almost surely erred in implying a uniform emotional style for preliterate peoples. Ong, *Orality and Literacy: The Technology of the Word* (London, 1982).

orbit.⁵⁶ A more recent suggestion, applied to twentieth-century society, is that romantic love is essential in an individualistic, market-oriented society, because of the irrationality of marriage yet society's need for procreation. This may seem more frivolous.⁵⁷ But the force of emotionology in various aspects of family life does remind us of the power of economically irrational behavior in modern history, some of which is spurred and justified by emotionology itself.

Even beyond family dynamics, emotionology has implications that must be explored. The decline in the acceptability of job-based anger for many workers in the twentieth century is no abstract issue, for a host of new techniques in personnel management, such as the retraining of foremen and personality testing, have arisen to express and deal with this change. These trends in themselves alter the work experience. They may relate to a decline in per capita rates of work-based protest.⁵⁸ From the viewpoint of employers, when worker discontent becomes emotionologically unacceptable and not just unacceptable in terms of economic implications or attacks on the power structure, mechanisms to deal with unrest, and reactions to unrest once it occurs, may need to be altered. Thus, that white-collar workers are held to different emotional standards than are blue-collar workers plays a role in the management of labor.⁵⁹ Certainly there is a fascinating, and linguistically quite precise, correspondence between mid-twentieth-century emotionological trends and Barrington Moore's claim that a decline in "moral indignation" has occurred in contemporary society.⁶⁰

Emotionology surely contributes to definitions of deviance. Expectations of a loving relationship between parents and children affected perceptions of wayward children in the nineteenth century. In our own day, we are urged to tighten our definitions of male sexual aggression in part because of new ideas about anger. The aggressive behaviors had not previously been approved as outlets for anger, but they were not so clearly labeled as unacceptably deviant as they are now.⁶¹

Emotionology may even relate to trends in the social history of health. Some anthropological evidence exists of a relationship between the most salient targets of disapproval in child rearing and the dominant diseases that arouse concern.⁶² If child rearing targets certain emotions for disapproval, might the effect not be explored in a mutual relationship to popular health concerns? Here the rise of new

⁵⁶ Goode, "Importance of Love," 38–41.

⁵⁷ S. M. Greenfield, "Love and Marriage in Modern America," *Sociological Quarterly*, 6 (1965): 361–77.

⁵⁸ De Swaan, "Politics of Agoraphobia."

⁵⁹ C. Wright Mills, *White Collar: The American Middle Classes* (New York, 1953); Mary C. H. Niles, *Middle Management: The Job of the Junior Administrator* (New York, 1941), 72–93; and William H. Whyte, Jr., *The Organization Man* (New York, 1956), 276.

⁶⁰ Moore, *Injustice: The Social Basis of Obedience and Revolt* (New York, 1978), 500–02. Richard Solomon has described efforts to rouse and manipulate anger, as part of new political protest, in a culture traditionally hostile to angry expression. See Solomon, *Mao's Political Revolution and the Chinese Political Culture* (Berkeley and Los Angeles, 1971), 289–94.

⁶¹ Much of the male liberationist literature of the past fifteen years criticizes the presumed sources of unacceptable male anger. For an explicitly feminist discussion, see Jean Baker Miller, *The Construction of Anger in Women and Men* (Wellesley, Mass., 1983), 5–7. The effort to extend the problems attributable to male anger is obvious in the currently fashionable explanation for sexual crimes. See Dolf Zillman and Jennings Bryant, "Pornography, Sexual Callousness, and the Trivialization of Rape," *Journal of Communication*, 14 (1982): 10–21.

⁶² Whiting and Child, *Child Training and Personality*.

levels of concern about anger in child rearing, particularly in recent decades, parallels the concern for “choleric” diseases of heart and arteries.

Changes in emotionology are related to other important shifts in a society. They describe not only formal intellectual outlook—which can be important in its own right—but also many of the expectations people have about their own emotional behavior, many of the disappointments they perceive at expectations unfulfilled, and many of the reactions they offer to the emotional behavior of others. Changes in the emotionology of anger, for example, easily spill over into reactions to the styles of religious or political leaders, even if actual emotional experience is in the most basic sense unaffected. Thus, in the twentieth-century United States, while anger in politics may not have diminished, its acceptable forms of expression have tightened, which affects the ability of the political process to convey intense feeling. This interaction between values and behavior most clearly distinguishes emotionology from the history of ideas and demonstrates the importance of emotionology to cultural history.

Emotionology also affects the targeting of emotions—the selection of what people or things are appropriate for particular emotional expressions—not only expressions of affection but also anger, fear, even greed. Again this effect can be independent of some actual change in emotional levels. In the eighteenth century, the emotion and expression of love were not necessarily more frequent or more intense than before, but they were concentrated more strictly on family—at least by men. Anger in the twentieth century has not necessarily declined, but, because of emotionological guidance enforced by institutional arrangements particularly in the workplace, men and women have sought new outlets. And shifts in emotional targets are important historical developments in their own right.

In sum, emotionology spurs a variety of personal and institutional arrangements, serving as a factor in definitions of leisure—witness the popularity of boxing at the turn of the century as an approved device for channeling anger⁶³—and politics as well as more intimate relationships. By coloring the way one’s own emotions and the emotions of others are judged, and by encouraging selection of some emotional targets over others, emotionology can serve as a powerful force in shaping the behavior of individuals and groups. Even changes in the ability to articulate emotional experiences—itsself an important development in recent centuries in Western society—alter the way life is perceived and experienced.⁶⁴

Finally, emotionology may well relate to emotion. If historians preserve the initial distinction between the two in their research strategies, they can test for correspondence, even though such correspondence cannot be assumed and may,

⁶³ American Institute of Child Life, *The Problem of Temper* (Philadelphia, 1914); and G. Stanley Hall, “A Study of Anger,” *American Journal of Psychology*, 10 (1899): 586.

⁶⁴ Thus, until the seventeenth century, much emotional distress, when perceived by upper-class patients, was reported as melancholia. In contemporary Western societies, this low-level sadness or anxiety is associated particularly with an uneducated minority. More “sophisticated” people rarely suffer from the rather general symptoms of melancholia, in part because of greater ability to articulate different aspects of emotional experience. This ability is itself an important product of emotionological change over the past several centuries. See Ong, *Orality and Literacy*; and MacDonald, *Mystical Bedlam*.

as suggested above, vary over time.⁶⁵ But emotion itself has a cognitive element, combining raw feeling—the “elementary subjective experience”—with judgment and perception, through which an individual evaluates, labels, and controls his feelings.⁶⁶ For example, research has shown that biologically similar generalized arousal states are labeled as different emotions depending on the subjects’ understanding of the situation that led to the arousal. If emotions were merely glandular or hormonal reactions, they might well not have a history, though even here reactions seem to vary with historically changing factors such as crowding. Despite continued dispute over the appropriate definition of emotions, a substantial consensus exists that emotions are not simply biological reactions but also involve an interplay between body and mind. This is why emotionology legitimately opens the way to a second phase in research, into emotional experience and its possible changes over time.

THE STUDY OF HISTORICAL CHANGE in emotional values and, to the extent possible and legitimate, in emotional experience itself is ripe for further examination. To be sure, some historians—both traditionalists in political history and practitioners of the new social history—will still prefer their subjects coldly rational, unswayed by considerations of passion.⁶⁷ But even the rationalists must be interested in emotionology as a guide to or manipulator of behavior. And, in fact, more is involved. Emotional experience may change over time, and emotional evaluations and targeting certainly change—in changing, they reflect important historical shifts and cause others. Hence, the inquiry, with the definitional distinctions that encourage precision, should proceed.

And this additional inquiry can and should range widely. Recent work has focused on love, understandably enough given our own emotional standards and the desirability of tracing their origins. But there is a need for fuller analysis of other emotions. The possibility of examining anger seems increasingly obvious, for, although the historians of love have tended to assume that anger decreases when recommendations for loving relationships increase, the emotionology of anger has had more interesting deviations than this formula can embrace. Fear, jealousy, joy, and greed merit assessment as well. The rise of an emphasis on love, for example, may have heightened the potential for jealousy, and demographic changes, reducing the number of children per family, may have had the same effect. Yet the desire to see the family as a warm emotional unit may have complicated the expression of jealousy—hence, among other things, the eventual modern fascination with sibling rivalry. Fear may have taken on new meaning, if not in actual experience at least in emotionology, as physical punishment declined and the threat of loss of love loomed larger. The declining use of bogeymen as

⁶⁵ Goode, “Importance of Love.”

⁶⁶ Kleinginna and Kleinginna, “Categorized List of Emotion Definitions,” 350; and Joseph R. Royce and Stephen R. Diamond, “A Multifactor-System Dynamics Theory of Emotion,” *Motivation and Emotion*, 4 (1981): 263–98.

⁶⁷ Gertrude Himmelfarb, “The Rule of Reason,” *Harper’s*, April 1984, pp. 84–90.

threats to children and the rise of surrogate sources of fear in twentieth-century children's media and even amusement parks offer considerable scope for historical assessment.⁶⁸ We have only begun to fathom the overall emotionological framework, even for modern Western history.

Emotionology also involves standards for emotional behavior outside as well as within the family. To date researchers have needlessly avoided examining emotional standards in work, recreation, and community relations, at least in studies of the eighteenth century onward. Nonfamilial emotions have received only passing comment in assessments of earlier periods—observations of high levels of angry bickering among neighbors or the affectionate attachments encouraged, before Protestantism, among male friends. Without question, the focus on family in more recent emotionology captures an important truth—that many modern people attempted to differentiate between family, which was a haven for emotional intensity, and other relationships, which were to be dominated by sweet reason. Yet at least implicit emotional standards affected nonfamily spheres. The shift in emotionology, toward a more intense focus on the family, almost surely played a role by the later nineteenth century in one key work area—growing confusion over the employment of domestic servants as all parties became less sure of appropriate emotional styles.⁶⁹ Some have argued, though mainly on the basis of formal literature, that a shift in the predominant emotional styles of male friendships accompanied the new emotionology of the family, a chord that echoes in some recent critiques of male style.⁷⁰ At the same time the relationship between the new family focus and acceptable emotions in women's friendships seems to have been different.⁷¹

As the larger emotional context is explored, even within the family, by examination of new valuations of emotions like anger and jealousy, the need for extension to extrafamilial settings becomes even more apparent. At what point, if ever, did the standards of anger control newly developed within the family extend to other relationships? When industrial psychologists like Elton Mayo tried to find new devices to limit (and to explain) anger in the workplace, were they not expressing their chagrin at the apparent disjuncture between middle-class emotional values and what they saw in the labor force?⁷² A full sense of emotionology and emotion alike dictates examination of changes in emotional standards in a wide variety of areas of social interaction.

⁶⁸ Delumeau, *La Peur en Occident*; and Roodenburg, "Autobiography of Isabella de Moerloose."

⁶⁹ David M. Katzman, *Seven Days a Week: Women and Domestic Servants in Industrializing America* (New York, 1978); Daniel E. Sutherland, *Americans and Their Servants: Domestic Service in the United States from 1800 to 1920* (Baton Rouge, La., 1981); Catherine E. Beecher, *Treatise on Domestic Economy* (Boston, 1841), 122, 134–40; and Faye E. Dudden, *Serving Women: Household Service in Nineteenth-Century America* (Middletown, Conn., 1983).

⁷⁰ Nelson, *Idea of Usury*, 139–63; Marc Fasteau, *The Male Machine* (New York, 1974); and Joseph Pleck and Jack Sawyer, eds., *Men and Masculinity* (Englewood Cliffs, N.J., 1974).

⁷¹ Carol Smith-Rosenberg, "The Female World of Love and Ritual: Relations between Women in Nineteenth-Century America," *Signs: Journal of Women in Culture and Society*, 1 (1975): 1–29.

⁷² Mayo, *The Human Problems of an Industrial Civilization* (New York, 1933), 84. For some similar reactions to workplace anger, see Frederick W. Taylor, *Shop Management* (New York, 1911), and *The Principles of Scientific Management* (New York, 1911).

By extending the aspects of emotional experience and range of emotions covered and, conjointly, by extending existing findings about emotionological change and its links to emotional experience, historians can, finally, contribute to a richer understanding of the past and a more sophisticated grasp of human psychology. Emotions have been inadequately studied in the behavioral sciences, after a flurry of interest following the work of Darwin and then Freud. Recent work reflects uncertainty about defining emotion itself and about identifying those emotions that are discrete and fundamental—hence, the call for historical inquiry into past psychological theories. But psychological theories do not reflect an adequate sense of the subjection of emotional values to the cultural factors of time and place. Thus, contemporary laboratory research into the experience of feeling anger reports uniform discomfort with the emotion.⁷³ Is this a constant, as the researchers assume, or the result of a particular culture, in which most anger is disapproved and bodily sensations that might be related to heart or arteries particularly feared? Why, in Homeric culture, could the sensation of anger be termed “sweeter than honey”?⁷⁴ Historians can play an active role in grasping a phenomenon demonstrably difficult to grasp and adequately described by no single theory of human behavior.

Much of the historical contribution lies in the future, despite the exciting and provocative start that has been made. Issues of periodization, of the range of emotions beyond love, and of standards for emotion beyond family largely remain to be explored. The concept of emotionology, introducing greater subtlety and greater initial caution into historical inquiry, seems at this point an essential methodological tool. Investigation of emotionological history is itself demanding and has great potential for illuminating aspects of the past and the linkage between past and present. Further probes beyond emotionology, into emotional change, may well be possible if premised on the two-step process of conceptualization and research.

⁷³ For a summary of commonplaces about the discomfort anger causes, and a brief suggestion of a corrective, see Donald K. Fromme and Clayton S. O'Brien, “A Dimensional Approach to the Circular Ordering of the Emotions,” *Motivation and Emotion*, 6 (1982): 343.

⁷⁴ “Sweeter wrath is by far than the honeycomb dripping with sweetener, and spreads through the hearts of men”; Homer *Iliad* 9.109. The abridged quotation is cited by G. Stanley Hall, with qualified approval, in his study of adolescence; Hall, *Adolescence*, 2 (New York, 1904): 370.

Engels, Marx, Malthus, and the Machine

JOHN M. SHERWOOD

HISTORIANS HAVE BEGUN OF LATE TO NOTE the influence of Friedrich Engels in his early writings on Karl Marx and on Marxism in general. Gareth Stedman Jones and Terrell Carver have traced the origin of many of Marx's ideas to the works of his colleague.¹ They have shown that Engels provided part of the theoretical foundation on which Marx built his view of the capitalist exploitation of labor. What has not been noted, until now, is that Engels developed in his first work, *The Condition of the Working Class in England*, theories of technological determinism and unemployment from which Marx derived his concepts of the industrial reserve army and the inability of a capitalist economy ever to improve the conditions of workers. Engels's book, furthermore, provided the original empirical foundation for the Marxist belief in the inevitability of revolution. What has also not been noted, until now, is that Engels espoused a theory of population growth derived from the much-denounced population theory of Thomas Malthus.

Engels published *Condition of the Working Class in England* in 1845, when he was only twenty-four. Born the son of a German textile manufacturer, Engels witnessed as a youth the changes wrought by early industrialization. In 1842 he was sent to Manchester to work in a British textile firm in which his father was a partner. He arrived "at almost the worst period of what was certainly the most catastrophic economic slump of the nineteenth century."² As one contemporary noted in 1843, "Never has the distress in the manufacturing towns been so severe, so penetrating, or so prolonged, as during the last two years. Never has

I am indebted to the Social Sciences and Humanities Research Council of Canada and to the Queen's University Advisory Research Committee for financial assistance that enabled me to carry out the research on this project. For references in the essay to works by Marx and Engels, see Karl Marx and Friedrich Engels, *Collected Works*, 22 vols. (New York, 1975-) [hereafter, *CW*]. Engels's *The Condition of the Working Class in England* is in volume 4. Unless otherwise noted, all quotations from Marx and Engels were taken from this collection; citations by volume and page number appear in parentheses immediately following each quotation.

¹ Jones, "Engels and the History of Marxism," in Eric J. Hobsbawm, ed., *The History of Marxism*, 1 (Bloomington, Ind., 1982), 316, and "Engels and the Genesis of Marxism," *New Left Review*, 106 (1977): 102; and Carver, *Marx and Engels: The Intellectual Relationship* (Bloomington, Ind., 1983), 50. For brief comments about the state of the literature on Engels, see Cecil L. Eubanks, *Karl Marx and Friedrich Engels: An Analytical Bibliography* (2d edn., New York, 1984), xiii, xxxvii-xlii.

² Eric Hobsbawm, "Introduction," in Friedrich Engels, *The Condition of the Working Class in England: From Personal Observations and Authentic Sources* (London, 1969), 14.

employment been so scarce, subsistence so scanty, destitution and disease so rife.”³ Precise statistics are difficult to obtain, but Sidney Pollard has reported that, in the manufacturing districts in 1842–43, 36 percent of the workers over the age of twelve were unemployed, 43 percent were employed part-time, and only 21 percent full-time. Some 20 percent of the population in industrial areas was considered to be destitute.⁴ In Manchester in 1842 about one-third of the families had pawned most of their personal possessions and required private charity as well as poor law aid in order to survive.⁵ The impressions Engels formed in Manchester of large-scale unemployment and of the degraded conditions of the workers colored his entire vision of the Industrial Revolution. For him, Manchester was the model of what was happening or would eventually happen to all workers.

Franz Mehring, Marx’s biographer, called Engels’s *Condition of the Working Class* “one of the foundation stones of socialism,” “an epoch-making work, the first great document of scientific socialism.” Lenin reported, “Everywhere Engels’s book began to be referred to as presenting the best picture of the conditions of the modern proletariat; and indeed, neither before 1845, nor after, has a single book appeared that presented an equally striking and true picture of the misery of the working class.” In 1969 the Marxist historian Eric J. Hobsbawm concluded, “Engels’ book remains today, as it was in 1845, by far the best single book on the working class of the period.” More recently, David McLellan, in fundamental agreement with Hobsbawm, wrote that “Engels’ descriptions can be taken, by and large, as probably the best piece of contemporary evidence that we have available to us.”⁶ Nonetheless, considering the place the book occupies in the development of Marxist theory and in Marxist historiography, *Condition of the Working Class* has received remarkably little scholarly attention.

In 1958 the social historians William O. Henderson and William H. Chaloner wrote an introduction to a new translation and critical edition of Engels’s work. They criticized its many factual errors and dismissed the methodology as faulty. “Engels was a brilliant political agitator,” they said, but “he was no historian.” His book was “a brilliant political tract, . . . a furious indictment of the English middle classes.” But Henderson and Chaloner’s critique is also flawed. Their chief focus

³ W. R. G., “Resources of an Increasing Population: Emigration or Manufactures,” *Westminster Review*, 40 (1843): 101–22, reprinted in J. M. Goldstrom, ed., *The Working Classes in the Victorian Age*, 2 (Westmead, Farnborough, Hants, 1973): 111.

⁴ Pollard, *A History of Labour in Sheffield* (Liverpool, 1959), 39. Also see Pollard, “Labour in Great Britain,” in Peter Mathias and M. M. Postan, eds., *The Cambridge Economic History*, volume 7: *The Industrial Economies*, pt. 1 (Cambridge, 1978), 123–29.

⁵ Joseph Adshead, *Distress in Manchester: Evidence of the State of the Labouring Classes in 1840–42* (London, 1842).

⁶ Mehring, *Karl Marx* (London, 1936), 105, 107; V. I. Lenin, “Frederick Engels,” in H. Pollitt, ed., *Lenin on Britain* (London, 1941), 19; Hobsbawm, “Introduction,” 17, and “History and the ‘Dark Satanic Mills,’” in Hobsbawm, *Labouring Men* (London, 1964), 105–19; and McLellan, *Engels* (Glasgow, 1977), 30. For Soviet and East European views of Engels’s work, see N. N. Stoskova, *Friedrich Engels über die Technik* (Leipzig, 1971); L. F. Ilyichov et al., *Frederick Engels: A Biography* (Moscow, 1974), 10–11, 58–63; T. I. Oizerman, *The Making of the Marxist Philosophy* (Moscow, 1981), 334–44; Henrich Gemkow et al., *Frederick Engels: A Biography* (Dresden, 1972); Horst Ullrich, *Der junge Engels: Eine historische-biographische Studie seiner weltanschaulichen Entwicklung in den Jahren 1834–1845*, 2 vols. (Berlin, G.D.R., 1961–66); Lev A. Leont’ev, *Engels und die ökonomische Lehre des Marxismus* (Berlin, G.D.R., 1970); and Alfred Kosing and Friedrich Richter, *Philosoph der Arbeiterklasse: Friedrich Engels, 1820–1870* (Berlin, G.D.R., 1971).

was not on Engels's interpretations but on his factual accuracy and handling of materials. As Asa Briggs caustically remarked, "If Engels sometimes garbled, they niggle."⁷

In 1974 the American literary critic Steven Marcus devoted a lengthy study to *Condition of the Working Class*, which he considered Engels's "best and most original work." Ignoring the questions usually posed by historians concerning factual and interpretative accuracy, Marcus wanted to employ the techniques of literary analysis to discover "what makes it outstanding among works of its kind and of its time." Historians Maxine Berg, Gertrude Himmelfarb, and Karel Williams have made the most recent contributions to this intermittent debate. Berg's analysis includes an excellent chapter placing Engels's ideas within the context of the discussion of the machinery question in Britain between 1815 and 1848. Himmelfarb analyzed Engels's contribution to the development during the Industrial Revolution of a new idea of poverty. Finally, from the point of view of semiotics, Williams discussed the "misreadings" of Engels's book by Chaloner, Henderson, Hobsbawm, and Marcus and attempted an Althusserian "reading" of the text, something Althusser himself had never done.⁸ All in all, this is a meager, and rather peculiar, scholarly harvest for "the first great document of scientific socialism." Summing up the situation, one commentator lamented, "Nobody seems to care that it is a book by Engels which has the distinction of being the first complete work in the Marxist canon."⁹

Raphael Samuel has called attention to the neglect of the historical phenomena that provided the basis for the development of Marx's theories—phenomena recorded by Engels in *Condition of the Working Class* that Marx employed. "The discussion of such questions," Samuel wrote, "has in recent years been left to the philosophers and the economists, each of them concerned, in their own way, with the theoretical consistency of Marx's texts rather than the industrial reality which he was attempting to dissect."¹⁰ This modern academic division of labor has particularly contributed to the neglect of Engels's book, because it is an exceptionally rich and complex work that involves a consideration of philosophy, economic theory, industrial, agricultural, urban, and demographic developments, as well as the Irish Question and the evolution of the labor movement. *Condition*

⁷ Friedrich Engels, *The Condition of the Working Class in England*, eds. Henderson and Chaloner (Oxford, 1958), xiii, xxii. Also see Chaloner and Henderson, "Friedrich Engels and the England of the 'Hungry Forties,'" in Institute of Economic Affairs, *The Long Debate on Poverty* (London, 1974), 169–86; W. O. Henderson, *The Life of Friedrich Engels*, 1 (London, 1976): 43–78; and Briggs, "The Chimney of the World," *New Statesman*, March 22, 1958, p. 379.

⁸ Marcus, *Engels, Manchester, and the Working Class* (New York, 1974), viii, 29; Williams, *From Pauperism to Poverty* (London, 1981), 278–302; Himmelfarb, *The Idea of Poverty: England in the Early Industrial Age* (London, 1984), 270–87; and Berg, *The Machinery Question and the Making of Political Economy, 1815–1848* (London, 1980), 315–42.

⁹ Werner J. Dannhauser, review of Marcus's *Engels, Manchester, and the Working Class*, in *Commentary*, November 1974, p. 97. Also see Fritz Nova, *Friedrich Engels: His Contributions to Political Theory* (New York, 1967), 90–92; and Jones, "Engels and the History of Marxism," 296–97.

¹⁰ Samuel, "Workshop of the World: Steam Power and Hand Technology in Mid-Victorian Britain," *History Workshop*, 3 (1977): 11. Also see Samuel, "British Marxist Historians, 1880–1980: Part One," *New Left Review*, 120 (1980): 21–22. For similar complaints concerning the treatment of the ideas of the classical economists, see Berg, *Machinery Question*, 3–7.

of the *Working Class* falls neatly into no one's bailiwick, and, therefore, no one has used it to examine the relationship between the development of industrialization, Engels's view of it, and the influence of his theories on the evolution of Marxist doctrines.

Most classical economists, unlike Engels, did not see technology as a principal cause of unemployment. They believed that the introduction of machinery, by reducing the cost of production, would ultimately benefit everyone. The cost of goods would come down; purchasing power would increase; production, therefore, would go up, not only in the industry in which machinery was introduced but also in every other industry. Thus, the general expansion of production in all industries would more than compensate for whatever jobs were lost by the introduction of machinery. A time lag might occur, but ultimately everyone in society would benefit from reductions in the cost of production and increases in the number of jobs.¹¹

By the 1840s, however, there was little evidence of improvement in the standard of living for the vast majority of the English people. On the contrary, the condition of workers in the cities was strong evidence of a decline. Periodic slumps in the economic cycle, such as the one occurring at the time of Engels's arrival in England in 1842, exacerbated conditions. Economists could deal theoretically with the problem of slumps by explaining them as a necessary part of the system and claiming, with some justice, that they would inevitably be followed by periods of increased production. Economists had more difficulty, however, accounting for the lack of improvement in the general standard of living and the apparent growth in the number of unemployed and destitute. They found a ready explanation in Malthus's theory of population growth, the indispensable ideological adjunct of classical economic theory and the origin of the iron law of wages.¹² It enabled its supporters to deny any responsibility for the economic conditions of the time and, with good conscience, to accuse workers of being the authors of their own fate. Malthus's theory is one of the best examples in modern history of the exploitation of a presumed natural law to mask the interests of a dominant order.

In his famous *Essay on the Principle of Population* (1798), Malthus argued that "population does invariably increase when the means of subsistence increase. And, that the superior power of population is repressed, and the actual population kept equal to the means of subsistence by misery and vice. Population, when unchecked, increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio." The inevitable result, he concluded, was an ever-recurring trend toward distress among the lower classes that could never be permanently alleviated. Nothing could be done to ameliorate their situation, because any improvement in their food supply would only lead them to have more children. Such considerations forced Malthus to the "disheartening reflection that the great obstacle in the way to any

¹¹ Berg, *Machinery Question*, 43–110; Alexander Gourvitch, *Survey of Economic Theory on Technological Change and Employment* (1940; reprint edn., New York, 1966), 39–48; and Mark Blaug, *Ricardian Economics* (New Haven, Conn., 1958), 64–79.

¹² S. Ambirajan, *Malthus and Classical Economics* (Bombay, 1959), 71; and Mark Blaug, *Economic Theory in Retrospect* (Homewood, Ill., 1968), 68.

extraordinary improvement in society is of a nature that we can never hope to overcome. The perpetual tendency in the race of man to increase beyond the means of subsistence is one of the general laws of animated nature which we can have no reason to expect to change." The principle of population, he believed, proved that "more will always be in want than can be adequately supplied." Malthus considered misery among the lower classes "an evil so deeply seated, that no human ingenuity can reach it." Only palliatives were possible, such as abolition of the poor laws, which caused an increase in the population and thereby created the very poor who had to be maintained.¹³

Reformers naturally attacked Malthus's doleful conclusions, which had been advanced as a refutation of the Enlightenment belief in progress. Not satisfied with his own argument, Malthus spent the rest of his life worrying it in revision after revision. He made some notable changes. First, he conceded that by means of late marriage, or what he termed "moral restraint," workers could hold down their numbers and thus improve their condition by demanding and receiving higher wages. Although this seems more hopeful, Malthus's conclusions in later editions are almost as pessimistic as those in the first. In the 1826 edition, the last one published in his lifetime, he reasserted that there was "a constant effort in the population to increase beyond the means of subsistence. This constant effort as constantly tends to subject the lower classes of society to distress, and to prevent any great permanent melioration of their condition."¹⁴

Malthus also continued to affirm his callous denial that human beings had any right to the means of subsistence. His most famous statement in this regard appeared in the second, supposedly more optimistic, edition of his essay (but was judiciously reworded in later ones): "A man who is born into a world already possessed, if he cannot get subsistence from his parents on whom he has a just demand, and if the society do[es] not want his labour, has no claim of right to the smallest portion of food, and, in fact, has no business to be where he is. At nature's mighty feast there is no vacant cover for him. She tells him to be gone, and will quickly execute her own orders, if he does not work upon the compassion of some of her guests."¹⁵

The second significant change in Malthus's thought was his recognition that in many circumstances workers were not responsible for their own poverty. As Joseph Spengler noted, Malthus perceived by the 1820s that effective demand for labor "tended to be realized only when proper moral and political conditions prevailed, when the social structure was elastic, when agricultural land holdings were adequately broken up, when commerce was active, when there were enough persons willing and able to consume more material wealth than they produced,

¹³ T. R. Malthus, *An Essay on the Principle of Population* (1798; reprint edn., New York, 1976), 56, 20, 24, 115, 99, 39–43.

¹⁴ Malthus, *An Essay on the Principle of Population* (1826; reprint edn., London, 1890), 14.

¹⁵ Malthus, *An Essay on the Principle of Population* (London, 1803), 531–32. This passage was quoted by Patricia James. See James, *Population Malthus* (London, 1979), 100. For Malthus's reaffirmation of the same idea in a later edition, see the 1826 edition of *Essay on the Principle of Population*, 476–77.

and when human wants were multiplying sufficiently to overcome the inelasticity of the demand for goods and services in terms of effort."¹⁶

Although Malthus had found, theoretically, a number of factors affecting the demand for labor, this discovery did not change his conviction that "the knowledge and prudence of the poor themselves are absolutely the *only* means by which any general and permanent improvement in their condition can be effected. They are really the arbiters of their own destiny; and what others can do for them is like the dust of the balance compared to what they can do for themselves."¹⁷ Thus, even though Malthus did hold out hope that workers could be taught to exercise moral restraint, limit the population and the competition for jobs, and improve their situation, the overwhelming impression he created over more than thirty years of public debate was uniformly pessimistic. In 1829 his fellow economist Nassau W. Senior observed that Malthus's principle of population had been "made the stalking-horse of negligence and injustice, the favourite objection to every project for making the resources of the country more productive." He thought that the majority of Malthus's readers overlooked the hopeful aspects of his ideas. "They seem to believe that the expansive power of population is a source of evil incapable not only of being subdued, but even of being mitigated."¹⁸ Malthus had succeeded in persuading manufacturers, politicians, and poor law reformers that nothing could be done to remedy the conditions of workers.¹⁹ They held to what could be described as vulgar Malthusianism, which affirmed that wages would always be driven down to subsistence level by population growth. The socialist leader Ferdinand Lassalle later called this theory the iron law of wages.²⁰

Historians have uncovered little evidence that poor relief caused workers to marry earlier, created overpopulation, or led to a decline in wages. Poor relief allowances were "selective, discontinuous, and supplementary . . . to other sources of income because they were typically too small to support an individual fully."²¹ The kind of oscillations in the birth rate predicted by Malthus, who thought

¹⁶ Spengler, "Malthus's Total Population Theory: A Restatement and Reappraisal," *Canadian Journal of Economics*, 11 (1945): 83–110, 234–64, esp. 99–100. Spengler's essay is still the best analysis of Malthus's later thought on population. Also see Samuel Hollander, "Malthus and the Post-Napoleonic Depression," *History of Political Economy*, 1 (1969): 306–35.

¹⁷ T. R. Malthus, *Principles of Political Economy* (1836; reprint edn., New York, 1968), 279, 426–30.

¹⁸ Senior, *Two Lectures on Population* (London, 1829), 89, 79, reprinted in Senior, *Selected Writings on Economics* (New York, 1966).

¹⁹ John R. Poynter, *Society and Pauperism: English Ideas on Poor Relief, 1795–1834* (London, 1969), 109; and Raymond G. Cowherd, *Political Economists and the English Poor Laws* (Athens, Ohio, 1977).

²⁰ Michael T. Wermel, *The Evolution of the Classical Wage Theory* (New York, 1939), 161–68.

²¹ Williams, *From Pauperism to Poverty*, 39; Mark Blaug, "The Myth of the Old Poor Law and the Making of the New," *Journal of Economic History*, 23 (1963): 151–84, and "The Poor Law Report Reexamined," *Journal of Economic History*, 24 (1964): 229–45; James P. Huzel, "Malthus, the Poor Law, and Population in Early Nineteenth-Century England," *Economic History Review*, 2d ser. [hereafter, *EHR*], 22 (1969): 430–52, and "The Demographic Impact of the Old Poor Law: More Reflections on Malthus," *EHR*, 33 (1980): 367–81; Osamu Saito, "Labour Supply Behaviour of the Poor in the English Industrial Revolution," *Journal of European Economic History*, 10 (1981): 633–52; J. D. Chambers, *Population, Economy, and Society in Pre-Industrial England* (London, 1972), 119–20; D. A. Baugh, "The Cost of Poor Relief in South-East England, 1790–1834," *EHR*, 28 (1975): 50–68; and Pollard, "Labour in Great Britain." For qualifications of the recent conclusions concerning the poor laws, see James S. Taylor, "The Mythology of the Old Poor Law," *Journal of Economic History*, 29 (1969): 292–97; and Donald McCloskey, "New Perspectives on the Old Poor Law," *Explorations in Economic History*, 10 (1973): 419–36.

reactions to economic distress would be immediate, did not occur.²² Moreover, population began to grow long before any notable change in the poor law late in the eighteenth century. E. A. Wrigley and R. S. Schofield have attributed approximately 70 percent of the increase in population in the eighteenth century to a pattern of earlier and more widespread marriage, a result of rising real income throughout the century. Thirty percent of the growth was attributable to a decline in mortality, which occurred, in particular, at the end of the century. The pattern—a century of increase in real wages followed by a decline in mortality—is not Malthusian.²³

Malthus, however, had so convinced his contemporaries of the importance of the population factor in maintaining low wages that few were willing to consider the impact of other factors, such as the introduction of machinery, on employment. At most, conceded David Ricardo, new machinery might have a temporary effect, but it was not the long-range and permanent cause of unemployment.²⁴ In contrast to Ricardo, Malthus held to the classical economists' view of machinery: "When a machine is invented, which, by saving labour, will bring goods into the market at a much cheaper rate than before, the most usual effect is such an extension of the demand for the commodity, by its being brought within the power of a much greater number of purchasers, that the value of the whole mass of goods made by the new machinery greatly exceeds their former value; and, notwithstanding the saving of labour, more hands, instead of fewer, are required in the manufacture." Malthus knew that the manufacturing system introduced greater risks of unemployment for many workers, but he dismissed the problem as merely "the unavoidable variations of manufacturing labour." He realized that in some situations savings produced by machinery might not be spent or invested, but he could see no danger of this at the time and concluded that there was "little reason to apprehend any permanent evil from the increase of machinery."²⁵

THE POSITION OF MALTHUS, RICARDO, and most classical economists was clear: it was not the introduction of machinery that caused the surplus population and unemployment but the sexual conduct or misconduct of the workers themselves. Others, however, believed that additional factors contributed to the problem, such as Irish immigration and the dispossession of small farmers by enclosures in the eighteenth century and the expansion of large-scale farming in the nineteenth. In sum, the sources of unemployment were believed to be four: displacement of

²² See the 1826 edition of *Essay on the Principle of Population*, 353.

²³ Wrigley and Schofield, *The Population History of England, 1541–1871* (Cambridge, Mass., 1981). Their conclusions concerning eighteenth-century population growth are more clearly expressed in two articles by Wrigley; see "The Growth of Population in Eighteenth-Century England: A Conundrum Resolved," *Past and Present*, 98 (1983): 131–50, and "Marriage, Fertility, and Population Growth in Eighteenth-Century England," in R. B. Outhwaite, ed., *Marriage and Society: Studies in the Social History of Marriage* (New York, 1981), 137–85. For the Malthusian elements, see E. A. Wrigley, "Malthus's Model of a Pre-Industrial Economy," in Jacques Dupâquier et al., eds., *Malthus Past and Present* (New York, 1983), 111–24.

²⁴ Blaug, *Ricardian Economics*, 64–74.

²⁵ See the 1826 edition of *Essay on the Principle of Population*, 356, and *Principles of Political Economy*, 352–60.

workers by machinery, increases in population caused by the poor laws or a temporary improvement in the standard of living, immigration from Ireland, and the displacement of agricultural workers. Engels was aware of all four factors, as passages in *Condition of the Working Class* demonstrate, but he believed that the ultimate cause of each was the introduction of machinery. Because he never addressed the question directly, however, his conclusions and presuppositions have to be pieced together from various sections of the book and from some of his other writings. An additional problem concerns the terminology Engels employed. A clarification of the terms that appear in *Condition of the Working Class* must precede an evaluation of the theory that the book expounds.

Engels's particular use of familiar terms has complicated the analysis of his beliefs concerning the relationship of technology to the displacement of workers. The problem begins in the preface and the opening sentence of the first chapter. In the preface, Engels claimed that he was the first writer to deal with "all the workers" and said that he "continually used the expressions working-men and proletarians, working class, propertyless class and proletariat as equivalents" (4: 303–04). Then, in the opening sentence of the first chapter, he declared, "The history of the proletariat in England begins with the second half of the last century, with the invention of the steam engine and of machinery for working cotton" (4: 307).

In spite of Engels's clear explication of his terminology, some writers have concluded that both Engels and Marx distinguished between the proletariat and the wage earner. Daniel Bell wrote, "For Marx, the proletariat was not identical with the masses of poor working people. . . . The classical proletariat consisted of factory workers whose class-consciousness was created by the conditions of their work." Even scholars who have studied the question closely have asserted that, in the term "proletariat," Marx and Engels included "only those wage earners who worked in large-scale industry powered by machinery and whose labor was necessarily cooperative in character. Thus being poor, or even a wage earner, did not make one a proletarian."²⁶

This misunderstanding stems from Engels's opening statement that "the invention of the steam engine and of machinery for working cotton" introduced "the history of the proletariat in England." This has been interpreted as Engels's attempt to distinguish between factory workers and those who were simply poor and propertyless. But, in fact, it is an assertion that the conditions of all workers in England in the 1840s—factory workers as well as the destitute—were the result of the introduction of the machine in the eighteenth century. For Engels, the term "proletariat" meant simply those who did not own their own means of production. In a preliminary draft for *The Communist Manifesto*, he wrote, "The proletariat is that class of society which procures its means of livelihood entirely and solely from the sale of its labour and not from the profit derived from any capital; whose weal

²⁶ Daniel Bell, *The Coming of Post-Industrial Society* (New York, 1973), 148; and Timothy McCarthy, *Marx and the Proletariat* (Westport, Conn., 1978), 78. Also see Hal Draper, *Karl Marx's Theory of Revolution*, 1 (New York, 1977), 129–67.

and woe, whose life and death, whose whole existence depend on the demand for labour, hence, on the alternation of times of good and bad business, on the fluctuations resulting from unbridled competition.”²⁷

Engels has been criticized for helping create “the myth of a pre-industrial golden age of harmonious working relationships” between employers and workers.²⁸ But this is to misinterpret the nature of the real myths that Engels created: first, that a propertyless working class had not existed before the introduction of the machine—“up to 1780,” he said elsewhere, “England had few proletarians” (3: 487)—and, second, that the entire British working class had become propertyless, that is, a proletariat.²⁹ Engels’s view of the composition of the proletariat was determined by his understanding of the nature and extent of factory work. His contemporaries differed in their definitions of a factory: some used the term only for those establishments with a common source of power such as water or steam; others used it for any establishment where workers were brought together, including what could be called workshops, whether they used machinery or not.³⁰ Engels seemed to believe that all factories used machinery, as indicated by his inclusion of glass works among his examples, even though this industry did not have mechanical improvements until after the 1840s.³¹ He believed, furthermore, that machine labor had “completely destroyed in all countries of the world . . . the old system of manufacture or industry founded upon manual labour” (4: 345). Given the widespread nature of the factory, and the purported destruction of manual labor, Engels naturally concluded that all workers in Britain had become proletarians. In fact, the purpose of *Condition of the Working Class* was to show how the machine had been responsible for the creation of the entire proletariat in England, that is, of all propertyless workers.

“The first proletarians,” Engels said, “were connected with manufacture, were engendered by it, and accordingly, those employed in manufacture, in the working up of raw materials, will first claim our attention.” The expansion of manufacture, he continued, resulted in the creation of another proletariat, the coal and metal miners. “Then, in the third place, manufacture influenced agriculture, and in the

²⁷ Friedrich Engels, “Principles of Communism,” in CW, 6: 341. In his letters to the *Rheinische Zeitung*, Engels referred to the proletarians as “the lower strata of society.” They were the “dispossessed,” “a class of unpropertied, absolutely poor people, a class which lives from hand to mouth, which multiplies rapidly, and which cannot afterwards be abolished, because it can never acquire stable possession of property”; CW, 2: 368–69, 373–74. In 1888 Engels was still using the same definition. See his note to the 1888 edition of *The Communist Manifesto*, where he defined the proletariat as “the class of modern wage-labourers who, having no means of production of their own, are reduced to selling their labour-power in order to live.” Karl Marx and Friedrich Engels, *Selected Works*, 1 (Moscow, 1955), 34 n.

²⁸ C. R. Dobson, *Masters and Journeymen: A Prehistory of Industrial Relations, 1717–1800* (London, 1980), 16.

²⁹ On the existence of propertyless workers before the Industrial Revolution, see C. Lis and H. Soly, *Poverty and Capitalism in Pre-Industrial Europe* (Brighton, Sussex, 1979). On preindustrial workers in England, see John Rule, *The Experience of Labour in Eighteenth-Century English Industry* (New York, 1981); Robert Malcolmson, *Life and Labour in England, 1700–1780* (London, 1980); T. S. Ashton, *An Economic History of England: The Eighteenth Century* (London, 1955), 201–35; and Christopher Hill, “Pottage for Freeborn Englishmen: Attitudes to Wage-Labour,” in Hill, *Change and Continuity in Seventeenth-Century England* (London, 1974), 219–38.

³⁰ Hsien-T’ing Fang, *The Triumph of the Factory System in England* (1930; reprint edn., Philadelphia, 1978), 213–14 n. 42. On the confusion about the meaning of these terms, see Herman Freudenberger and Fritz Redlich, “The Industrial Development of Europe: Reality, Symbols, Images,” *Kyklos*, 17 (1964): 372–401.

³¹ Fang, *Triumph of the Factory System*, 19.

fourth, the condition of Ireland; and the fractions of the proletariat belonging to each, will find their place accordingly" (4: 324). Because he believed that the entire proletariat had been created by the machine, Engels was able to use the expressions "working-men and proletarians, working class, propertyless class and proletariat as equivalents." For the same reason, he attributed the conditions of workers almost anywhere in the British Isles to the impact of machinery.

In *Condition of the Working Class*, Engels devoted one chapter to "Factory Hands"—primarily workers employed in the cotton industry—and another chapter to "The Remaining Branches of Industry," including stocking weaving, lace making, cotton printing, metal working, pottery, glass making, and dress-making. The purpose of the second chapter, he wrote, was "to record how far the factory system has succeeded in forcing its way into each branch of industry" (4: 479). Occasionally he did indicate where handicraft methods had survived, but the effect of these exceptions was virtually destroyed in the conclusion, where he stated that his examination

testifies to the gradual but sure introduction of the factory system into all branches of industry, recognizable especially by the employment of women and children. I have not thought it necessary to trace in every case the progress of machinery and the superseding of men as workers. Every one who is in any degree acquainted with the nature of manufacture can fill this out for himself. . . . In all directions machinery is being introduced, and the last trace of the working-man's independence thus destroyed. In all directions the family is being dissolved by the labour of wife and children, or inverted by the husband's being thrown out of employment and made dependent upon them for bread; everywhere inevitable machinery bestows upon the great capitalist command of trade and of the workers with it (4: 497).

If Engels's first mistake was to equate factory labor with machine labor, his second mistake, which compounded the problem, was to equate the employment of women and children with factory labor. Wherever he found either factory work or employment of women and children, he presupposed, without further examination, that machinery had been introduced. He seems to have been unaware that factories had developed before the introduction of steam power and that many factories used little or no machinery. Workers were also brought together in order to increase the division of labor and improve supervision of the work force.³² Engels was equally unaware of the employment history of women and children. He apparently derived his ideas from Peter Gaskell, who wrote a book in 1833 about the effects of machinery in the cotton industry. Gaskell believed that in manufacturing districts "there [was] no employment to be found for adult males," because their places had been taken by women and children.³³ Although

³² On eighteenth-century English factories, see Ashton, *Economic History of England*, 113–17. On similar developments in the United States, see Claudia Goldin and Kenneth Sokoloff, "Women, Children, and Industrialization in the Early Republic: Evidence from the Manufacturing Censuses," *Journal of Economic History*, 43 (1982): 742–43.

³³ Gaskell, *The Manufacturing Population of England* (1833; reprint edn., New York, 1972), 184–85, and *Artisans and Machinery: The Moral and Physical Conditions of the Manufacturing Population* (1836; reprint edn., New York, 1968), 172. Gaskell's *Artisans and Machinery* is a revised and enlarged edition of his *Manufacturing Population of England*.

a modern historian has said that "no one supposes child labour to have been a creation of the factory system,"³⁴ Engels, in fact, believed precisely that. He did not know, for example, that large numbers of children in Birmingham and elsewhere were employed in workshops that used little or no machinery.³⁵ As John Hammond and Barbara Hammond noted, "Scarcely any evil associated with the factory system was entirely new in kind. In many domestic industries the hours were long, the pay was poor, and children worked from a tender age." Marjorie Cruickshank has found "plenty of evidence" that the situation of children outside the cotton mills was much worse.³⁶

Similarly, in her classic study of women and the Industrial Revolution, Ivy Pinchbeck wrote, "It is often assumed that the woman worker was produced by the Industrial Revolution. . . . This theory is, however, quite unsupported by facts." Pinchbeck showed that for centuries women had been engaged in handicraft work in the home and had been expected to earn their own keep. According to Pinchbeck, the development of factories probably led to a decline in the employment of married women by eliminating some of the domestic industries in which they had traditionally worked. Thus, married women were not taking jobs away from men, nor were factories leading to a breakup of the home. Pinchbeck concluded that the Industrial Revolution caused a decline in married women's economic position but immensely improved their domestic conditions by permitting them to devote their time to household duties and the care of children.³⁷

Engels was led by his preconceptions about factories and the employment of women and children to magnify out of all proportion the degree to which machinery was transforming British industry. Again his ideas seem to have come from Gaskell, who wrote in 1833 that what he said about the cotton industry could be applied to all other industries. "The universal application of steam power" was destroying domestic labor in all industries, packing the population into towns, and destroying families. Engels and Gaskell were not alone in their beliefs. In 1844 William Cooke Taylor said, "Every branch of industry in England, and to some

³⁴ Rule, *Experience of Labour*, 43.

³⁵ Eric Hopkins, "Working Hours and Conditions during the Industrial Revolution: A Re-Appraisal," *EHR*, 35 (1982): 54-55.

³⁶ Hammond and Hammond, *The Town Labourer* (1917; reprint edn., London, 1966), 31; and Cruickshank, *Children and Industry: Child Health and Welfare in North-West Textile Towns during the Nineteenth Century* (Manchester, 1981), 48. There is no authoritative monograph on the employment of children during the Industrial Revolution, but the topic has been dealt with in many books. See Ivy Pinchbeck and Margaret Hewitt, *Children in English Society*, 2 vols. (London, 1969-73), esp. 2: chap. 14; Clark Nardinelli, "Child Labor and the Factory Acts," *Journal of Economic History*, 40 (1980): 739-55; and E. H. Hunt, *British Labour History, 1815-1914* (London, 1981), 9-17.

³⁷ Pinchbeck, *Women Workers and the Industrial Revolution, 1750-1850*, 1 (1930; reprint edn., London, 1969): 307. For an expansion of Pinchbeck's argument, see Eric Richards, "Women in the British Economy since 1700: An Interpretation," *History*, 59 (1974): 337-57. Richards concluded that rates of women's participation in the labor market did not reach preindustrial levels until the middle of the twentieth century. In the United States, which lacked a large domestic industry, industrialization increased the work available for women. See Goldin and Sokoloff, "Women, Children, and Industrialization." In some areas, however, married women were eventually displaced by machine production. See William Mulligan, "The Family and Technological Change: The Shoemakers of Lynn, Massachusetts, during the Transition from Hand to Machine Production, 1850-1880" (Ph.D. dissertation, Clark University, 1982).

extent throughout Europe and America, is daily assuming more and more definitely the aspect of factory organisation.”³⁸

Engels's notion that machinery was taking over industry appears throughout his book. “The victory of machine-work over hand-work in the chief branches of English industry . . . and the history of the latter from that time forward simply relates how the hand-workers have been driven by machinery from one position to another” (4: 312). The lower middle class, according to Engels, was being eliminated. It was “an undenied and easily explained fact that the numerous petty middle class of the ‘good old times’ has been annihilated by manufacture, and resolved into rich capitalists on the one hand and poor workers on the other” (4: 429). The machine's destructive capabilities became a refrain. “The history of cotton manufacture,” Engels repeated, “is the story of improvements in every direction, most of which have become domesticated in the other branches of industry as well. Hand-work is superseded by machine-work almost universally, nearly all manipulations are conducted by the aid of steam or water, and every year is bringing further improvements” (4: 429).

Any reader of Engels, or of his contemporaries for that matter, would have had to conclude that machinery was invading every branch of British industry and would soon eliminate, if it had not already done so, the last vestiges of handicraft work. Studies have conclusively shown that this picture of technological change was as overdrawn as Engels's view of machinery's impact on women and children. First of all, despite his claim, Engels never dealt with “all the workers.” With the exception of milliners and seamstresses, he ignored laborers in London, “by far the biggest centre of manufactures in the whole country.” There work was still dominated by artisans.³⁹ In addition, there is no mention in his book of tailors, shoemakers, locksmiths, stonemasons, printers, watchmakers, jewelers, house-painters, cabinet makers, shipbuilders, or construction and transportation workers. Moreover, even in some of the industries he discussed, machinery did not replace hand work until much later in the century.⁴⁰ As Engels himself recognized in an introduction to the 1892 edition of his book, many skilled tradesmen, far from being reduced to poverty, had been able to protect themselves very well during the second half of the century, forming trade unions, raising wages, and becoming an aristocracy of labor.⁴¹

Both Marx and Engels were misled by too great a reliance on classic studies of the impact of the machine. Engels wrote that the introductory section of his *Condition of the Working Class*, on the origin of the proletariat, was “chiefly” taken from Gaskell (4: 366 n.). An examination of the two authors shows that Engels was

³⁸ Gaskell, *Manufacturing Population of England*, 9–10; and Taylor, *Factories and the Factory System* (London, 1844), 111, as quoted in Fang, *Triumph of the Factory System*, 22. Also see Archibald Alison, *Principles of Population*, 1 (London, 1840): 188.

³⁹ A. E. Musson, “The British Industrial Revolution,” *History*, 67 (1982): 258. Also see Musson, *The Growth of British Industry* (London, 1978), 71, 115–16; I. J. Prothero, *Artisans and Politics in Early Nineteenth-Century London* (Folkestone, Kent, 1979), 20; and David Goodway, *London Chartism, 1838–1848* (Cambridge, 1982).

⁴⁰ Samuel, “Workshop of the World”; Prothero, *Artisans and Politics*, 338; Eric J. Hobsbawm, *Industry and Empire* (Baltimore, 1969), 71–72; and Berg, *Machinery Question*, 20–31.

⁴¹ Hobsbawm, “Introduction,” 31.

also strongly influenced by Gaskell's analysis of the impact of machinery, which Gaskell predicted would in the near future replace almost all labor, agricultural as well as industrial.⁴² In the work of Andrew Ure, the great defender of the machine and the manufacturers, Marx read that the introduction of the self-acting spinning mule had caused the disappearance of male spinners and the hiring of adolescents and children. Ure's conclusions seem to have been based partly on manufacturers' blurbs, partly on the logic of the machine, which simplified work procedures, and partly on the actual displacement of men. But William Lazonick has shown that adult males continued to dominate mule spinning until the 1960s, when the technology finally became obsolete. Far from providing, as Marx believed, the classic example of workers eliminated by the introduction of automatic machinery, the male spinners formed after 1850 "the best organized and the best-financed union in all of Britain."⁴³

In addition to overestimating the degree to which male workers were displaced and traditional handicrafts rendered obsolete, Engels failed to realize that the general expansion in the economy was leading to an expansion in the number of workers employed in producing and distributing consumer goods and providing services. "The expanding contingent of cotton workers created increased demand for the output of shoemakers, chandlers, bricklayers and many other specialities."⁴⁴ Instead of abolishing the lower middle class, the economic development underway led to an expansion of the petty bourgeoisie, who were employed in new types of work—what has been called the "service revolution."⁴⁵ Although less dramatic, it was a change in the composition of the work force as important as that directly caused by the Industrial Revolution.

Although Engels was aware of the impetus that economic development had given to the creation of jobs in new trades and other industries, he failed to take sufficient account of it.⁴⁶ He also failed to note that economic development could lead to growth in traditional trades. The expansion of hand-loom weaving until 1830 is the classic example of this phenomenon.⁴⁷ Furthermore, the rapid

⁴² Gaskell, *Manufacturing Population*, 340–41. Also see his *Artisans and Machinery*, 310–62.

⁴³ Lazonick, "Industrial Relations and Technical Change: The Case of the Self-Acting Mule," *Cambridge Journal of Economics*, 3 (1979): 231–32, and "Conflict and Control in the Industrial Revolution: Social Relations in the British Cotton Factory," in Robert Weible et al., eds., *Essays from the Lowell Conference on Industrial History* (Lowell, Mass., 1981), 17; and John Foster, *Class Struggle and the Industrial Revolution* (London, 1974), 83, 231.

⁴⁴ Robert Glen, *Urban Workers in the Industrial Revolution* (London, 1984), 94.

⁴⁵ Prothero, *Artisans and Politics*, 2, 23; Ashton, *Economic History of England*, 216–17; R. M. Hartwell, "The Service Revolution: The Growth of Services in Modern Economy," in Carlo M. Cipolla, ed., *The Fontana Economic History of Europe*, volume 3: *The Industrial Revolution* (Glasgow, 1973), 358–96; Neil McKendrick, "Home Demand and Economic Growth: A New View of the Role of Women and Children in the Industrial Revolution," in McKendrick, ed., *Historical Perspectives: Studies in English Thought and Society* (London, 1974), 152–210; and Musson, *Growth of British Industry*, 129–42.

⁴⁶ A. E. Musson, "Technological Change and Manpower," *History*, 67 (1982): 240, and "The Engineering Industry," in Roy Church, ed., *The Dynamics of Victorian Business* (London, 1980), 87–106; David S. Landes, *The Unbound Prometheus* (London, 1969), 118–19; Maxine Berg et al., "Manufacture in Town and Country before the Factory," in Berg et al., eds., *Manufacture in Town and Country before the Factory* (Cambridge, 1983), 11–12.

⁴⁷ Duncan Bythell, *The Handloom Weavers: A Study in the English Cotton Industry during the Industrial Revolution* (Cambridge, 1969). On other industries, see Bythell, *The Sweated Trades: Outwork in Nineteenth-Century Britain* (London, 1978); and James A. Schmiechen, *Sweated Industries and Sweated Labor: The London Clothing Trades, 1867–1914* (Champaign, Ill., 1983).

expansion in the number of workers required by industry led to a dilution of traditional labor controls and increased competition among workers, which in turn created a new political consciousness among artisans and handicraftsmen.⁴⁸ Engels mistakenly attributed this new political activity primarily to factory workers. Factory hands, he said, have "formed the nucleus of the Labour Movement" (4: 324). But, in looking for a new designation for labor developments in the early part of the nineteenth century, one could make a good case for describing the period as the "Age of the Artisan" rather than the "Age of the Proletariat." As William H. Sewell, Jr., has said, all recent analyses of the working class during this time agree on one point: "Skilled artisans, not workers in the new factory industries, dominated labor movements during the first decades of industrialization."⁴⁹

Technological and economic historians recently have reinterpreted the significance of the steam engine and the spinning jenny for the Industrial Revolution. What might be called Engels's great machine theory of history, the analogue of his contemporary Thomas Carlyle's great man theory of history, was overthrown decades ago by Paul Mantoux and the early students of handicraft industries, who emphasized the long period of preparation required for the Industrial Revolution.⁵⁰ New studies by technological and economic historians have further diminished the significance of the steam engine and spinning jenny. Instead of seeing one or two major innovations as revolutionizing society, historians have emphasized the importance of the incremental changes that occurred in a broad variety of industries.⁵¹ G. N. von Tunzelmann wrote that "output per head rose more through a host of small-scale changes stemming from practical experimentation at every level than through a handful of big breakthroughs." The economic historian Donald McCloskey told us that "the industrial revolution was not the Age of Cotton or of Railways or even of Steam entirely; it was the age of improvement."⁵²

Nonetheless, there can be no denying the impact that the steam engine and the spinning jenny had on the contemporary imagination. The many small changes were, cumulatively, at least as important as the major ones, but the major changes—some radically new—caught public attention. In 1842 William Cooke Taylor remarked, "The steam-engine had no precedent, the spinning-jenny is without ancestry, the mule and the power-loom entered on no prepared heritage;

⁴⁸ Prothero, *Artisans and Politics*, 43.

⁴⁹ Sewell, *Work and Revolution in France* (Cambridge, 1980), 1, 285 n. 1. For a study of the factors affecting the development of class consciousness among workers, see Foster, *Class Struggle and the Industrial Revolution*.

⁵⁰ Mantoux, *The Industrial Revolution in the Eighteenth Century* (1928; reprint edn., New York, 1961); and Conrad Gill, *The Rise of the Irish Linen Industry* (1925; reprint edn., 1964), 3–4.

⁵¹ Peter Mathias, "Skills and the Diffusion of Innovations from Britain in the Eighteenth Century," in Mathias, *The Transformation of England* (New York, 1979), 21–44; and Berg *et al.*, "Manufacture in Town and Country," 10. For the United States, see George Daniels, "The Big Questions in the History of American Technology," *Technology and Culture*, 2 (1970): 10–11.

⁵² Von Tunzelmann, "Technical Progress during the Industrial Revolution," in Roderick Floud and Donald McCloskey, eds., *The Economic History of Britain since 1700*, 1 (Cambridge, 1981): 163; McCloskey, "The Industrial Revolution, 1780–1860: A Survey," in *ibid.*, 118. Also see von Tunzelmann, *Steam Power and British Industrialization to 1860* (Oxford, 1978).

they sprang into sudden existence like Minerva from the brain of Jupiter.”⁵³ Because of the novelty and prominence of new machines, critics were inclined, if not driven, to attribute to them all the changes they saw. Marx and Engels were no different from their contemporaries in this regard; they were simply more logical and systematic in their evaluations. This kind of technological determinism underlay one of Marx’s most famous quotations: “The hand-mill gives you society with the feudal lord; the steam-mill, society with the industrial capitalist.”⁵⁴ The point here is not to debate Marx’s and Engels’s technological determinism but to pinpoint the early development of Engels’s theories. That he tended to attribute almost all change to the effects of the machine is clear from his treatment of its influence on unemployment and population growth. Moreover, his idea that the machine was responsible for unemployment is simply a corollary of his belief that it was responsible for the transformation of almost all workers into proletarians.

TO ENGELS, THE MACHINE CAUSED UNEMPLOYMENT in two ways: directly, because it was labor saving and threw large numbers of people out of work; indirectly, because by encouraging economic and technological growth the machine also allowed population growth. Machinery, he said, should be the cause of rejoicing. But “every improvement in machinery throws workers out of employment, and the greater the advance, the more numerous the unemployed; each great improvement produces, therefore, upon a number of workers the effect of a commercial crisis, creates want, wretchedness, and crime.” The jenny, for example, produced six times as much as the spinning wheel. “Thus every new jenny threw five spinners out of employment.” In like manner, he said, “whole armies” have been thrown out of work by the mule because of the increase in the number of spindles. Engels contended that, if one asked workers begging on corners or selling matches what they used to do, they would reply, “Mill hands thrown out of work by machinery” (4: 429–33).

Engels’s assumption about the consequences of the jenny and mule was typical of analysts of machine productivity at the time. Although their logic was clear, it was also false. Total employment in the cotton industry (hand workers and factory workers) continued to expand throughout most of this period. In an article for a German journal in 1844, Engels wrote, “With the aid of the machine a child of eight was now able to produce more than twenty grown men before. Six hundred thousand factory workers, of whom half are children and more than half female,

⁵³ Taylor, *Notes of a Tour in the Manufacturing Districts of Lancashire* (London, 1842), 4–6, as quoted in E. P. Thompson, *The Making of the English Working Class* (Baltimore, 1968), 208.

⁵⁴ Marx, *The Poverty of Philosophy*, in CW, 6: 166. On Marx and technological determinism, see William H. Shaw, *Marx’s Theory of History* (Stanford, 1978), and “‘The Handmill Gives You the Feudal Lord’: Marx’s Technological Determinism,” *History and Theory*, 18 (1979): 155–76; Gerald A. Cohen, *Marx’s Theory of History* (Oxford, 1978); Melvin Rader, *Marx’s Interpretation of History* (Oxford, 1979); Nathan Rosenberg, “Marx as a Student of Technology,” in Rosenberg, *Inside the Black Box* (Cambridge, 1982), 34–54; and Gary Young, “The Fundamental Contradiction of Capitalist Production,” *Philosophy and Public Affairs*, 5 (1976): 196–234. Also see the articles by Richard W. Miller, Philippe Van Parijs, and James Noble, in Terence Ball and James Farr, eds., *After Marx* (Cambridge, 1984), 59–119.

are doing the work of one hundred and fifty million people" (3: 482). The figures do not add up and should not be taken literally. But they conjure up an image of the magnitude of the unemployment that Engels was trying to convey. In Engels's view the machine, if directly responsible for creating "whole armies" of unemployed, was also indirectly responsible for an expansion of the population to the point of surplus. To those who argued that without the new system of factory production the millions of additional workers could not have found employment, Engels angrily retorted, "As though the bourgeois did not know well enough that without machinery and the expansion of industry which it produced, these 'millions' would never have been brought into the world and grown up" (4: 432-33).

Here Engels used the classic Malthusian explanation for the expansion that more than doubled the British populace from 8,900,000 in 1781 to 18,555,000 in 1841: boom periods in the economy led to a shortage of labor and a rise in wages, inducing workers to marry earlier and have more children, thereby creating a surplus of workers, whose competition reduced wages and increased unemployment. And yet Engels rivaled Marx in his denunciation of Malthus's principle, calling it "this vile, infamous theory, this hideous blasphemy against nature and mankind"—"the crudest, most barbarous theory that ever existed" (3: 437, 420). Such statements have led Marxist and non-Marxist writers, who agree on little else, to describe the contrast between Malthusian and Marxist population theories as irreconcilable. The demographer William Petersen spoke of Marx's "total rejection of Malthus" and told us that Marxists "believe that the rational control of human fertility is iniquitous." The Marxist economist Paul Sweezy summed up this consensus when he said that Marx "had no use whatever for the Malthusian theory or any of its variants."⁵⁵ No one, except the British Marxist Ronald Meek, has discussed Engels's ideas on population, and Meek, too, has presupposed Engels's agreement with Marx. Like most writers, Meek usually refers to "Marx and Engels" as if the names are a single proper noun.⁵⁶

Yet it is clear from *Condition of the Working Class*, and other works, that Engels accepted Malthus's mechanism of population change, though not his policy

⁵⁵ Petersen, "Marx versus Malthus: The Symbols and the Men," in Petersen, *The Politics of Population* (London, 1964), 73-74; and Sweezy, *The Theory of Capitalist Development* (1942; reprint edn., New York, 1970), 86. Also see Herman E. Daly, "A Marxian-Malthusian View of Poverty and Development," *Population Studies*, 25 (1971): 25-37; H. L. Beales, "The Historical Context of the Essay On Population," in D. V. Glass, ed., *Introduction to Malthus* (London, 1953), 7-8; and Joel Mokyr, "Malthusian Models and Irish History," *Journal of Economic History*, 40 (1980): 160. The Belgian Marxist scholar Ernest Mandel, who tried to dissociate Engels and Marx from Malthusian ideas, said that they "never upheld" Ferdinand Lassalle's theory of the iron law of wages, which Marx said was fundamentally "the Malthusian theory of population." In fact, however, Engels, in a note to the 1885 edition of *The Poverty of Philosophy*, claimed to have been the first one to formulate the thesis that "the 'natural,' i.e., normal, price of labour power coincides with the wage minimum, i.e., with the equivalent in value of the means of subsistence absolutely indispensable for the life and procreation of the worker." Marx had accepted his analysis, Engels said, and Lassalle had subsequently taken it over from both of them. Engels, by his own account, was the father of the iron law of wages. Mandel, *The Formation of the Economic Thought of Karl Marx* (New York, 1971), 140-43; Marx and Engels, *Selected Works*, 2: 29; and Marx and Engels, *CW*, 6: 125 n.

⁵⁶ Meek, "Malthus—Yesterday and Today," *Science and Society*, 18 (1954): 31-39, and *Marx and Engels on Malthus* (New York, 1954).

recommendations. On occasion, Engels was explicit in stating his agreement, conceding that Malthus's law had "a good deal of truth in it under existing conditions" (4: 570). He added:

If there are too few labourers on hand, prices, i.e., wages, rise, the workers are more prosperous, marriages multiply, more children are born and more live to grow up, until a sufficient number of labourers has been secured. If there are too many on hand, prices fall, want of work, poverty, and starvation, and consequent diseases arise, and the "surplus population" is put out of the way. . . . Malthus . . . was . . . right, in his way, in asserting that there is always a "surplus population"; that there are always too many people in the world; he is wrong only when he asserts that there are more people on hand than can be maintained from the available means of subsistence. (4: 480, 566, 570–72).

Astonishingly, Engels even agreed with the conclusions of the Poor Law commissioners that the Old Poor Law was "a check upon industry, a reward for improvident marriage, a stimulus to increased population, and a means of counterbalancing the effect of an increased population upon wages; a national provision for discouraging the honest and industrious, and protecting the lazy, vicious, and improvident." Engels commented:

This description of the action of the Old Poor Law is certainly correct; relief fosters laziness and increase of "surplus population." Under present social conditions it is perfectly clear that the poor man is compelled to be an egotist, and when he can choose, living equally well in either case, he prefers doing nothing to working. But what follows therefrom? That our present social conditions are good for nothing, and not as the Malthusian Commissioners conclude, that poverty is a crime, and, as such, to be visited with heinous penalties which may serve as a warning to others (4: 572).

Engels thus adopted completely the Malthusian explanation of population growth, at least in existing society. He need not have done so. The theory was by no means universally accepted. A contemporary, George R. Porter, with whose work Engels was familiar, attributed the increase in population to a substantial decline in the death rate rather than an increase in the birth rate, which he thought, in fact, had also declined. From this Porter concluded that the quality of life must have improved.⁵⁷

Even some classical economists did not totally accept Malthus's explanation. On the basis of continuous improvement in living conditions in all civilized nations, Nassau Senior argued, *pace* Malthus, that the means of subsistence naturally increase faster than the population. "If it be conceded, that there exists in the human race a tendency to rise from barbarism to civilization, and that the means of subsistence are proportionally more abundant in a civilized than in a savage state, and neither of these propositions can be denied, it must follow that there is a natural tendency in subsistence to increase in a greater ratio than population."⁵⁸

⁵⁷ Porter, *The Progress of the Nation* (London, 1836), 18–20.

⁵⁸ Senior, *Two Lectures on Population*, 49. For criticisms of Malthus and alternative explanations of population growth, see James A. Field, *Essays on Population and Other Papers* (Chicago, 1931); E. P. Hutchinson, *The Population Debate: The Development of Conflicting Theories up to 1900* (Boston, 1967); Kenneth Smith, *The Malthusian Controversy* (London, 1951); and J. F. McCleary, *The Malthusian Population Theory* (London, 1953).

Although Engels never discussed the theories of Senior and Porter, he was obviously unable to accept either of their explanations for the growth of population. He could not believe that the conditions he had seen in Manchester and elsewhere constituted progress. If so, what could earlier conditions possibly have been? Rather, he wrote, Malthus's critics could not "gainsay the facts which have impelled Malthus to his principle" (3: 438). He thought that Gaskell's picture of a decline in the standard of living since the eighteenth century was closer to the facts (4: 366), and Malthus's theory was the necessary complement that helped explain that decline. Engels accepted the theory for the same reason manufacturers did: it supported his own ideological commitment. But it also confirmed his belief in the necessity of revolution, which he had held before coming to Britain. Convinced by the German communist Moses Hess that England was headed for a social revolution, Engels, from the moment he arrived, "had eyes for nothing but the signs of approaching revolution."⁵⁹ On November 30, 1842, a few weeks after he reached Manchester, Engels declared, "Revolution is inevitable for England" (2: 373). Malthus provided the only contemporary theory that accorded with his belief in revolution, at least until Marx developed an independent theory to account for unemployment, a variation of Engels's ideas.

Clearly Engels's endorsement of Malthus's theory of population did not include acceptance of Malthus's conclusion that little could be done to improve the conditions of workers. It was this notion and Malthus's policy recommendations, not his analysis of the mechanism of population change in a capitalist society, that Engels considered "vile" and "barbarous." In an article written at the same time as *Condition of the Working Class*, Engels explained his position. He had been persuaded by the arguments of Archibald Alison, whom he called "one of the most able economists and statisticians," that each man was capable of producing more than he needed to survive—otherwise, he would never have been able to support his children, and society would never have been raised to the current level of civilization. Alison had argued that increased application of capital and labor could enormously increase food supply.⁶⁰ Engels added that scientific knowledge was cumulative and increased at least as much as population. It also progressed in a sort of geometric ratio. He believed that science could add indefinitely to man's productive power; no problem was insoluble if scientific knowledge was applied to it (3: 436–40). Engels concluded that Malthus's fundamental principle concerning the ratio between the means of subsistence and population was wrong, even though his facts concerning the existing social arrangements were not. Malthus, he believed, had failed to see that surplus population was a result of surplus wealth, which created surplus productive power. Engels erred in saying Malthus had also failed to see that population presses on the means of employment, not on the means of subsistence. He conceded that Malthus's argument—

⁵⁹ Gustav Mayer, *Friedrich Engels* (New York, 1936), 31; Norman Levine, *The Tragic Deception: Marx contra Engels* (Santa Barbara, Calif., 1975), 123–24; and Richard N. Hunt, *The Political Ideas of Marx and Engels*, 1 (Pittsburgh, 1974): 105.

⁶⁰ Alison, *Principles of Population*, 33–82.

that labor power, being subject like other commodities to the law of competition, was exposed to periodic crises and fluctuations—had “merit.” For Engels, Malthus’s law, thus corrected, did hold for the capitalist system of production.

LET US RETURN NOW TO THE FOUR FACTORS (enumerated above) that were believed to be the sources of proletarian unemployment. It is now clear that the first two were one for Engels: the introduction of machinery both displaced workers and increased the population. But what of the other two possible sources of unemployment—Irish immigration and the displacement of agricultural workers? Engels thought that these, too, were ultimately reducible to the introduction of machinery. The view of Irish immigration as an exogenous factor primarily responsible for conditions in England was held by a number of writers, including three whom Engels respected—Archibald Alison, Thomas Carlyle, and Dr. James Kay, secretary of the Manchester Board of Health. Engels quoted Carlyle, for example, as saying that the Irishman was “the sorest evil this country has to strive with. In his rags and laughing savagery, he is there to undertake all work that can be done by mere strength of hand and back; for wages that will purchase him potatoes. . . . The Saxon man, if he cannot work on those terms, finds no work. . . . The uncivilised Irishman . . . drives out the Saxon native, takes possession in his room. There abides he, in his squalor and unreason, in his falsity and drunken violence, as the ready-made nucleus of degradation and disorder.”⁶¹

In contrast to Alison, Carlyle, and Kay, Engels regarded the Irish not as extraneous to but as part of the English economic system. “The rapid extension of English industry could not have taken place if England had not possessed in the numerous and impoverished population of Ireland a reserve at command” (4: 389). The repeal of the Corn Laws in England, he noted, might reduce the cost of food, stimulate production, and lead to reemployment, but only temporarily. “The ‘surplus’ population of England, and especially of Ireland, is sufficient to supply English manufacture with the necessary operatives, even if it were doubled” (4: 566). The temporary improvement in workers’ conditions would increase the population, setting the stage for the next crisis.

But what was the origin of the surplus Irish population? It is hard to see how the conditions of the Irish, either in Ireland or in England, could be the result of technological unemployment. Most of them had never been employed in a factory, and, as Engels categorically stated, they were “utterly unfit for manufacture as now conducted” (4: 560). It is also difficult to discern how the Malthusian mechanism could have affected population growth in Ireland. Supposedly a rise in the wages paid to workers, particularly mill hands, caused them to marry earlier and have more children, but the Irish were not factory laborers and thus not receiving higher wages. Alternatively, the Poor Law encouraged workers to be improvident and have children when they could not afford them, but there was no Poor Law

⁶¹ Carlyle, *Chartism* (London, 1840), 28.

in Ireland until 1838. Nevertheless, Engels maintained that the introduction of the machine in England brought into play the Malthusian process of population growth in Ireland. After explaining how the introduction of machinery produced the English proletariat, he wrote, "Ireland had entered upon an orderly development only since the beginning of the 18th century. There, too, the population . . . now rapidly multiplied, especially after the advance in manufacture began to draw masses of Irishmen towards England" (4: 321). The Irish were not, according to Engels, alone responsible for poverty in England (as Alison, Carlyle, and Kay had thought), but as part of the British economy they had become participants in the Malthusian cycle set in motion by the introduction of the machine.

Displaced agricultural workers were yet another category of the unemployed whose fate Engels attributed to the introduction of the machine. The invention of the spinning jenny, he asserted, created a great demand for weavers, which increased wages and led weavers to give up their other occupation as farmers. Their land was turned over to a new class of large tenant farmers able to improve the yield of the land and undersell the yeoman farmer, who either had to take to spinning or weaving himself or become an agricultural laborer. If the spinning jenny had created the industrial proletariat, "the same machine gave rise to the agricultural proletariat" (4: 311). The patriarchal relationship between laborers and employers dissolved as workers became day laborers, solely dependent on wages for subsistence. The invention of agricultural machinery also had an adverse effect on rural employment:

The constant extension of farming on a large scale, the introduction of threshing and other machines, and the employment of women and children (which is now so general that its effects have recently been investigated by a special official commission) threw a large number of men out of employment. It is manifest, therefore, that here, too, the system of industrial production had made its entrance, by means of farming on a large-scale. . . , by the introduction of machinery, steam, and the labour of women and children" (4: 550).

Engels derived this explanation as well from Gaskell, who wrote:

Agriculture is undergoing a transition as great, and almost as remarkable, as manufacture. . . . Mechanical contrivances for lessening human labour, are sought for with as great avidity in the one case as in the other. . . . The same causes are at work, therefore, upon the two great divisions of national industry, and their effects have even been more severely felt by the agricultural than the manufacturing labourer; and have in great measure, already pauperized the whole body, and nearly extinguished the peasantry, as a moral and independent class of the community.⁶²

Both Gaskell and Engels ignored the enclosure movement and its effect on the creation of a rural proletariat, which had been going on for centuries. The number of workers employed in agriculture had increased from about 1,350,000 in 1750 to 2,100,000 in 1851.⁶³ Historians now recognize that the proletariat began to grow

⁶² Gaskell, *Manufacturing Population*, 339–40, 35–46.

⁶³ Eric L. Jones, ed., *Agriculture and Economic Growth in England, 1650–1815* (London, 1967), 23–24; Pollard, "Labour in Great Britain," 142; J. D. Chambers, "Enclosure and Labour Supply in the Industrial Revolution,"

long before the Industrial Revolution. Charles Tilly recently calculated that by 1750 the majority of people in Western Europe were already proletarians.⁶⁴

Gaskell and Engels also greatly overestimated the impact of machinery on farming. Until the second half of the nineteenth century, the mechanical thresher was the only machine that improved agricultural productivity. Raphael Samuel concluded that "the agricultural revolution of the 18th and early 19th centuries had nothing to do with machinery."⁶⁵ Furthermore, the surplus of laborers clearly resulted more from the increase in population than from the displacement of male workers by women and children. In fact, women had traditionally worked in the fields, and their employment may have declined, rather than increased, during this period. The change from pastoral farming to cereal production, which required heavier labor, and the competition of surplus male farm laborers led to a displacement of women. The introduction of machinery in the second half of the century enabled landowners to dispense with the labor of women and children.⁶⁶

In his analyses of unemployment among agricultural workers as well as among Irish and industrial workers, Engels compressed demographic and economic changes that took place over a period of a century or more into one monumental event, the Industrial Revolution, caused by one inexorable factor, the machine. The changes that he interpreted were caused by a variety of factors, about which historians still disagree, but Engels attributed all of them to the introduction of the steam engine and spinning jenny. It was an overwhelming vision of the world, still captivating today. Everywhere he found that the machine was the force turning industrial and agricultural workers alike into proletarians afflicted by progressive impoverishment and immiseration. Any improvement in their conditions must ultimately lead to a renewal of the Malthusian cycle.

EHR, 5 (1953): 319–43. Recently, historians have qualified Chambers's conclusion that population increase rather than enclosure provided the growing labor supply for industry. They have pointed out that increased efficiency of production caused a decline in the relative number of agricultural laborers required to produce food and thus indirectly released other laborers to work in industry. Such qualifications, however, do not invalidate Chambers's main point that enclosure was not the immediate source of new industrial labor. See R. D. Baack and R. P. Thomas, "The Enclosure Movement and the Supply of Labour during the Industrial Revolution," *Journal of European Economic History*, 3 (1974): 401–23. Also see James A. Yelling, *Common Field and Enclosure in England, 1450–1850* (London, 1977), 222–27; P. K. O'Brien, "Agriculture and the Industrial Revolution," *EHR*, 30 (1977): 166–81; N. F. R. Crafts, "Enclosure and Labor Supply Revisited," *Explorations in Economic History*, 15 (1978): 172–83; and "Income Elasticities of Demand and the Release of Labour by Agriculture during the British Revolution," *Journal of European Economic History*, 9 (1980): 153–68; and Michael Turner, *Enclosures in Britain, 1750–1830* (London, 1984), 76–80.

⁶⁴ Tilly, "Demographic Origins of the European Proletariat," in David Levine, ed., *Proletarianization and Family History* (Orlando, Fla., 1984), 33.

⁶⁵ Samuel, "Village Labour," in Samuel, ed., *Village Life and Labour* (London, 1975), 17; David H. Morgan, "The Place of Harvesters in Nineteenth-Century Village Life," in *ibid.*, 29, and *Harvesters and Harvesting, 1840–1900: A Study of the Rural Proletariat* (London, 1982), 15; J. D. Chambers and G. E. Mingay, *The Agricultural Revolution, 1750–1880* (London, 1966), 2–3; E. J. T. Collins, "Harvest Technology and Labour Supply in Britain, 1790–1870," *EHR*, 22 (1969): 453–73; and John R. Walton, "Mechanization in Agriculture: A Study in the Adoption Process," in H. S. A. Fox and R. A. Butlin, eds., *Change in the Countryside: Essays on Rural England, 1500–1900* (London, 1979), 23–42.

⁶⁶ K. D. M. Snell, "Agricultural Seasonal Unemployment, the Standard of Living, and Women's Work in the South and East, 1690–1860," *EHR*, 34 (1981): 407–37; Eric L. Jones, *Agriculture and the Industrial Revolution* (Oxford, 1974), 101; Jennie Kitteringham, "Country Work Girls in Nineteenth-Century England," in Samuel, *Village Life and Labour*, 131–32; and Michael Roberts, "Sickles and Scythes: Women's Work and Men's Work at Harvest Time," *History Workshop*, 7 (1979): 3–28.

Since the existing system offered no hope, the only alternative for Engels was revolution. The Malthusian law of population led industrialists to believe that nothing could be done for workers, whose only salvation lay in "moral restraint." But it also persuaded Engels that nothing could be done for workers within the existing system. Proletarians must accomplish their own salvation—not by sexual abstinence but by revolution. Engels spoke of "the deep wrath of the whole working class, from Glasgow to London, against the rich, by whom they are systematically plundered and mercilessly left to their fate, a wrath which before too long a time goes by, a time almost within the power of man to predict, must break out into a revolution in comparison with which the French Revolution, and the year 1794, will prove to have been child's play" (4: 323). Here, too, Engels echoed Gaskell, who predicted that the displacement of men by machinery would lead to an "explosion . . . from which the imagination turns with sickening terror."⁶⁷

ENGELS'S IMPRESSIONS OF THE EFFECTS of the introduction of machinery helped create an image of the Industrial Revolution whose popular influence is evident even today: the rapid and widespread introduction of machinery led to the universal establishment of the factory system, which employed large numbers of women and children for the first time and created both massive technological unemployment and a rebellious proletariat.⁶⁸ Engels's technological determinism formed the basis of Marx's theories of the inevitability of the industrial reserve army and the inability of the capitalist economy to improve the conditions of workers. And, in *Condition of the Working Class*, Marx found the empirical foundation for his belief in the inevitability of revolution. Marx, who frequently praised Engels's work, called it the "first precise study" of the proletarian movement.⁶⁹

The Marxist economists Ronald Meek and Paul Sweezy have said that, in contrast to the classical economic theorists, Marx put "technological change at the very center of economic theory."⁷⁰ Maxine Berg has shown that the classical economists were deeply concerned with the machinery question, but, insofar as Meek and Sweezy are correct, it was Engels, not Marx, who originally placed the overwhelming emphasis on technology. Marx arrived at his conclusions concerning the historic revolutionary role of the proletariat through a study of Hegel's philosophy of history and his exposure to French socialist thought in Paris. It was "a philosophical deduction rather than a product of observation."⁷¹ At the time that Engels published *Condition of the Working Class*, Marx had little knowledge of

⁶⁷ Gaskell, *Manufacturing Population*, 341.

⁶⁸ For a useful critique of this image and its creation, see Krishan Kumar, *Prophecy and Progress: The Sociology of Industrial and Post-Industrial Society* (Baltimore, 1978).

⁶⁹ Marx, as quoted in Alan Gilbert, *Marx's Politics: Communists and Citizens* (New Brunswick, N.J., 1981), 51.

⁷⁰ See Meek's commentary on Sweezy's "Karl Marx and the Industrial Revolution," in Robert V. Eagly, ed., *Events, Ideology, and Economic Theory* (Detroit, 1968), 121.

⁷¹ Leszek Kolakowski, *Main Currents of Marxism*, 1 (Oxford, 1978): 130, 373–75; and Ali Rattansi, *Marx and the Division of Labour* (London, 1982), 60–66.

technology and industrial development.⁷² Engels developed his ideas from the classical economists and the British working-class movement. His contributions to Marxism—the emphasis on technology and the explanation of its role in creating the industrial reserve army—were theories he derived not only from Gaskell but also from the Owenite and labor critique of machinery.⁷³

The theory of the revolutionary proletariat is generally considered to be the weakest link in the Marxist system. Meek remarked that there is an unresolved problem in Marx's idea of economic development: "Just because you bring dynamic technological change into your basic model of development, it does not necessarily follow that you thereby see the system as tending inexorably towards overthrow and ruin. Just because your model predicts impoverishment, it does not necessarily follow that you believe that the impoverished workers will eventually rise up and put an end to the system. It would seem that something more must be brought into our picture if we are to be able to give a Marxian explanation of Marx's revolutionary political conclusions."⁷⁴ Therefore, Meek wondered, how had Marx reached his revolutionary conclusions?

The answer to Meek's unresolved problem lies not so much in theory as in Engels's experience of the English working class, which confirmed his earlier expectations of revolution. He arrived at a disastrous time in English history, saw strikes and protests everywhere. The Irish, he believed, had infused stolid English workers with their rebellious spirit. It was Engels who provided the flesh and blood examples for Marx's primarily philosophical conception of the revolutionary proletariat.

Engels created the deterministic framework. He identified the machine as the moving force producing a surplus population and preventing the improvement of conditions, and he predicted the revolutionary outcome of the process. As Bob Rowthorn has said, "Much of Marx's more famous economic writings are really an attempt to systematize, under the influence of Ricardian thought, the earlier insights of Engels."⁷⁵ Over the following twenty years Marx read widely in the literature on technology and industry. He added considerable detail to the picture presented by his associate, modified some of the ideas, gave a far better historical explanation of the development of capitalism, and elaborated a more sophisticated explanation of the functioning of the system, but he did not change Engels's basic framework. That is why he could write so enthusiastically to Engels in 1863 about *Condition of the Working Class*. After comparing the book again to his notes on the

⁷² Marx, of course, was aware of the importance of technology, but it was not one of the focal points of his analysis. For the best picture of his approach at that time, see "Economic and Philosophic Manuscripts of 1844," *CW*, 3: 229–346. He was concerned primarily with the problems of private property, the division of labor, and alienation. For the most comprehensive treatment of the evolution of Marx's and Engels's ideas before 1848, see Edward D. Gregory, "The Influence of French and English Socialism on the Early Thought of Friedrich Engels and Karl Marx, 1835–1847" (Ph.D. dissertation, Queen's University, Kingston, Ontario, 3 vols., 1978).

⁷³ Berg, *Machinery Question*, 322–23.

⁷⁴ See Meek's commentary in Eagly, *Events, Ideology, and Economic Theory*, 123; and Paul M. Sweezy, "Marx and the Proletariat," in Sweezy, *Modern Capitalism and Other Essays* (New York, 1972), 148.

⁷⁵ Rowthorn, *Capitalism, Conflict, and Inflation* (London, 1980), 227 n. 15.

following period, Marx concluded, "As far as the main points . . . are concerned, they have been confirmed down to the smallest detail by developments since 1844."⁷⁶

The Communist Manifesto, in particular, contains Marx's adaptation of Engels's earlier ideas.⁷⁷ There is the same expectation that revolution is imminent, the belief that nine-tenths of the population had been proletarianized, and the conviction that the wages of the working classes were being driven down to subsistence level. Revolution would come about because of the inability of the system to provide for workers. Originally formulated to prove the necessity of private property, the Malthusian population doctrine proved to Marx and Engels the necessity for revolution. Malthus was the unwitting godfather of the revolutionary proletariat. As Marx said:

The modern labourer . . . , instead of rising with the progress of industry, sinks deeper and deeper below the conditions of existence of his own class. He becomes a pauper, and pauperism develops more rapidly than population and wealth. And here it becomes evident, that the bourgeoisie is unfit any longer to be the ruling class in society. . . . It is unfit to rule because it is incompetent to assure an existence to its slave within his slavery, because it cannot help letting him sink into such a state, that it has to feed him, instead of being fed by him. Society can no longer live under this bourgeoisie, in other words, its existence is no longer compatible with society (4: 495–96).

The Communist Manifesto, however, also contains Marx's introduction of a change in Engels's theory of technological determinism and the creation of the proletariat. By 1848 Engels and Marx had become aware that factories did not necessarily employ machinery. In a draft for *The Communist Manifesto*, Engels recognized the continued existence of handicrafts, which were being organized in large workshops similar to factories (4: 342). Therefore, factories were not simply the product of the machine. By 1867 Marx also knew that wage laborers had existed before the introduction of machinery, that is, that machinery had not been the original cause of the creation of the modern proletariat. In *Capital*, he noted the early development of large workshops under the control of capitalists in which the mode of production was similar to that of medieval craftsmen. Marx thought that this period extended roughly from the middle of the sixteenth century to the last third of the eighteenth. There was, however, one major difference between the medieval and modern shop—the intense division of labor in the capitalist system. It was this division of labor that ultimately led to the development of machines, because the work of individual craftsmen had been so simplified that it could be performed by machines. In his analysis, Marx shifted the emphasis from the mode of production to the social relations of production, thereby considerably attenuating Engels's technological determinism.⁷⁸

⁷⁶ Marx to Engels, April 9, 1863, in Karl Marx and Frederick Engels, *On Britain* (Moscow, 1962), 539.

⁷⁷ Carver, *Marx and Engels*, 78.

⁷⁸ Marx, *Capital*, 3 vols. (Chicago, 1909–10), 1: 353, 369, 404. For a discussion of Marx's emphasis on social relations, see Donald MacKenzie, "Marx and the Machine," *Technology and Culture*, 25 (1984): 482–86.

The second major change Marx introduced concerned Engels's theory of unemployment. As we saw, Engels's explanation of unemployment had both a technological and a Malthusian component. Marx accepted Engels's explanation of technological unemployment. He did not so much reject the Malthusian elements—or presuppose their validity, as a number of authors have suggested—as consider them to be inadequate to explain the growth of the industrial reserve army.⁷⁹ Like the Malthusians, Marx believed that an increase in wages would increase the marriage and the birth rates among workers, as well as reduce the death rate among children, but he also believed that poverty itself caused the workers to have children more rapidly than they would otherwise. The factory system needed large numbers of young people as laborers, which encouraged early marriage and the production of children. The greater the poverty, the greater the need to produce children. Thus, “the absolute size of the families stands in inverse proportion to the height of wages, and therefore to the amount of means of subsistence of which the different categories of labourers dispose.” For Marx, then, population increase was a natural development, but he considered the increase to be insufficient for the insatiable needs of capital during periods of expansion. In order for capital to develop in an unfettered fashion, it needed an additional supply of laborers, which it created by extending the hours of work, by hiring women and children, by introducing capitalist methods into agriculture and displacing farm workers, and, most important, by creating technological unemployment. “Capitalist production can by no means content itself with the quantity of disposable labour-power which the natural increase of population yields. It requires for its free play an industrial reserve army independent of these natural limits.”⁸⁰

Marx's contribution to Engels's theory of technological unemployment was to explain the process that prevented the expansion of capital from employing all available workers, a possibility Engels had accounted for by means of Malthus's population principle. For his new theory Marx was also indebted in part to Malthus, Ricardo, and to John Barton, a lesser-known economist who published an attack in 1817 on the classical economists' position on machinery.⁸¹

Malthus was among the first to see the inadequacies of his original formulation of the principle of population. He realized that it was not subsistence that determined population growth but the demand for labor, which was itself determined by the growth and investment of capital. Unfortunately, Malthus discovered, capitalists did not always invest or spend their profits, which caused insufficient demand, consumption, and employment. “The greatest powers of production,” he said, “are rendered comparatively useless without effectual

⁷⁹ Samuel M. Levin, “Marx vs. Malthus,” *Papers of the Michigan Academy of Science, Arts, and Letters*, 22 (1936): 243–58. For a slightly different version of Levin's essay, see his *Malthus and the Conduct of Life* (New York, 1967), 89–107. Also see Petersen, “Marx versus Malthus,” 72–89; and Daly, “A Marxist-Malthusian View,” 25–37.

⁸⁰ Marx, *Capital*, 1: 601–02, 706, 595; 3: 256, 298–99. For Marx's most extensive comments on population, see his *Grundrisse: Foundations of the Critique of Political Economy* (Baltimore, 1973), 604–10. For an attempt to turn Marx's fragmentary comments on population into a systematic theory, see Michio Morishima and George Catephores, *Value, Exploitation, and Growth* (London, 1978), chap. 5.

⁸¹ Marx explicitly acknowledged his debt to Ricardo and Barton; *Capital*, 1: 692 n., 445 n.

consumption, and . . . a proper distribution of the produce is as necessary to the continued increase of wealth as the means of producing it." But, because merchants and manufacturers spent so much of their time engaged in business, they did not have the leisure to spend all that they earned. "There must therefore be a considerable class of persons who have both the will and the power to consume more material wealth than they produce, or the mercantile classes could not continue profitably to produce so much more than they consume."⁸² Malthus in effect advocated a division of labor in which some classes devoted themselves to accumulation and others to consumption. He thought that landlords, clergymen, statesmen, and lawyers, as well as their servants, admirably performed this most useful and necessary function of consuming without producing.

Marx ridiculed this defense of parasitic classes⁸³ and only rarely acknowledged the debt he owed to Malthus, but he did take note of Malthus's theory of underconsumption. In *Capital*, Marx wrote that the consuming power of society is "restricted by the tendency to accumulate, the greed for an expansion of capital. . . . To the extent that the productive power develops, it finds itself at variance with the narrow basis on which the conditions of consumption rest." He concluded that "the last cause of all crisis remains the poverty and restricted consumption of the masses as compared to the tendency of capitalist production to develop the productive forces in such a way, that only the absolute power of consumption of the entire society would be their limit."⁸⁴ One of the factors counterbalancing the tendency toward crises of underconsumption was the existence of Malthus's unproductive consumers. In a passage that could almost have been written by Malthus himself, Marx said: "Over against this overproduction on one side must be placed overconsumption on the other, production for the sake of production must be confronted by consumption for the sake of consumption. What the industrial capitalist has to surrender to landlords, the State, creditors of the State, the church, and so forth, who only consume revenue, is an absolute diminution of his wealth, but it keeps his lust for enrichment going and thus preserves his capitalist soul."⁸⁵

Joan Robinson believed that Marx foreshadowed the modern Keynesian theory of effective demand—that is, "a low propensity to consume, caused by unequal distribution of income and inordinate thrift, is a necessary condition for rapid accumulation in the early stages of capitalism, but, when its work has been done, it impedes accumulation, by reducing the incentive to invest, and generates recurrent and ever-deepening slumps."⁸⁶ Robinson's interpretation has been questioned,⁸⁷ but there can be no question of Marx's debt to Malthus for his theory of underconsumption. According to Marx, "the real contribution made by Malthus

⁸² Malthus, *Principles of Political Economy*, 411.

⁸³ Marx, *Capital*, 1: 653.

⁸⁴ Marx, *Capital*, 3: 286–87.

⁸⁵ Marx, *Theories of Surplus Value*, 3 vols. (Moscow, 1963–71), 2: 578; 1: 283.

⁸⁶ Robinson, "Marx on Unemployment," *Economic Journal*, 51 (1941): 248. Also see Robinson, *An Essay on Marxian Economics* (London, 1966), 48–51, 70–72.

⁸⁷ Blaug, *Economic Theory*, 16, and *Ricardian Economics*, 238–40.

... is that he places the main emphasis on the *unequal* exchange between capital and labour, ... whereas Ricardo ... leaves the origin of surplus-value obscure.”⁸⁸ From Malthus, Marx learned that there was no automatic mechanism to reestablish harmony between capitalists and workers. Demand could be defective with dire consequences for the workers as well as for the capitalist system. From Ricardo came the admission that there were circumstances in which machinery could lead to unemployment. To the annoyance of other classical economists, Ricardo declared, “The opinion entertained by the labouring class that the employment of machinery is frequently detrimental to their interests, is not founded on prejudice and error, but is conformable to the correct principles of political economy.”⁸⁹

To the development of Marx’s thought, Malthus and Ricardo contributed concessions, recognitions of the limitations of the principles they accepted. What John Barton contributed was a new principle. Barton argued that the increase of wealth in the state did not necessarily lead to an increase in employment, as Adam Smith and others contended. He observed that a greater and greater proportion of the national wealth was being invested in fixed capital, in machinery, which reduced the proportion available for circulating capital and the amount devoted to the employment of workers and, thus, reduced opportunities for employment. It was conceivable for national wealth to be increasing—even for the absolute number of workers employed in industry to be increasing—at the same time that the relative employment possibilities for workers were decreasing.⁹⁰ This became the basis of Marx’s own explanation of the creation of the industrial reserve army.

Marx believed that Barton’s and Ricardo’s analyses overthrew “the whole of the absurd theory of population ... , in particular the claptrap of the vulgar economists,” that workers should strive to reduce their numbers to counterbalance the lack of demand for labor. Barton and Ricardo had shown that “to keep down the labouring population, thus diminishing the supply of labour, and consequently, raising its price, would only accelerate the application of machinery, the conversion of circulating into fixed capital, and, hence, make the population artificially ‘redundant.’” Thus, for Marx, although population growth might contribute to the misery of workers, the restriction of population offered no relief, for higher wages only induced employers to introduce more machinery. The essential factor causing the degradation of workers was machinery, not population growth. “The labouring population,” Marx said, “always increases more rapidly than the conditions under which capital can employ this increase for its own

⁸⁸ Marx, *Theories of Surplus Value*, 3: 14. Also see his *Grundrisse*, 417–20. There were other writers who espoused theories of underconsumption, but Marx apparently singled out Malthus because his ideas pointed to the concept of surplus value. For several other favorable references to Malthus, see Jean Freville, *La misère et le nombre: L’épouvantail Malthusien* (Paris, 1956), 244–45. For the arguments against Marx as an underconsumptionist, see Ernest Mandel, *Marxist Economic Theory*, 1 (New York, 1970): 361–71; Joseph A. Schumpeter, *Capitalism, Socialism, and Democracy* (New York, 1950), 38–39; and Thomas Sowell, *Marxism: Philosophy and Economics* (New York, 1985), 84–95.

⁸⁹ Ricardo, *Principles of Political Economy* (London, 1891), 383–84. On Ricardo’s contentious revision, see Berg, *Machinery Question*, 43–74.

⁹⁰ Barton, *Observations on the Circumstances which Influence the Condition of the Labouring Classes of Society* (1817; reprint edn., Baltimore, 1934), 17–18.

self-expansion." Ultimately, the workers would become rebellious and revolt. In a passage echoing Engels a quarter of a century earlier, Marx concluded that, along with the growth of capital,

grows the mass of misery, oppression, slavery, degradation, exploitation; but with this too grows the revolt of the working class, a class always increasing in numbers, and disciplined, united, organised by the very mechanism of the process of capitalist production itself. The monopoly of capital becomes a fetter upon the mode of production, which has sprung up and flourished along with, and under it. Centralisation of the means of production and socialisation of labour at last reach a point where they become incompatible with their capitalist integument. This integument is burst asunder. The knell of capitalist private property sounds. The expropriators are expropriated.⁹¹

Despite the nearly universal belief that Marx and Engels would have nothing to do with any idea derived from Malthus, both of their theories of unemployment were influenced by him. Engels incorporated the population principle of Malthus the demographer, and Marx took the doctrine of effective demand of Malthus the economist. Malthus helped confirm both Engels's and Marx's belief in the inevitability of revolution. Engels believed that Malthus's theory proved the hopelessness of improvement in the condition of workers; Marx used Malthus's idea that the capitalist system would not automatically provide effective demand for all the labor available.

Combining the ideas of Malthus, Ricardo, and Barton, Marx argued that the proportion of constant to variable capital would always increase and reduce the relative demand for labor. "The relative mass of the industrial reserve-army increases . . . with the potential energy of wealth. But the greater this reserve army in proportion to the active labour-army, the greater is the mass of a consolidated surplus-population, whose misery is in inverse ratio to its torment of labour. The more extensive, finally, the lazarus-layers of the working class, and the industrial reserve army, the greater is official pauperism. This is the absolute general law of capitalist accumulation."⁹²

⁹¹ Marx, *Capital*, 1: 708, 836–37.

⁹² *Ibid.*, 1: 681–89, 707. A number of authors have argued that, by the time of *Capital*, Marx no longer believed in the immiseration of the working classes. See Thomas Sowell, "Marx's 'Increasing Misery' Doctrine," *American Economic Review*, 50 (1960): 111–20; Martin Nicolaus, "Proletariat and Middle Class in Marx: Hegelian Choreography and the Capitalist Dialectic," in James Weinstein and David W. Eakins, eds., *For a New America* (New York, 1970), 252–83; Mandel, *Marxist Economic Theory*, 150–54, and *Formation of the Economic Thought of Karl Marx*, 140–53; Roman Rosdolsky, *The Making of Marx's 'Capital'* (London, 1977), 282–313; Michael Harrington, *The Twilight of Capitalism* (New York, 1976), 130–34; and Isaac D. Balbus, *Marxism and Domination* (Princeton, N.J., 1982), 48–55. For restatements of the traditional view, see Ronald L. Meek, "Marx's 'Doctrine of Increasing Misery,'" *Science and Society*, 26 (1962): 422–41; Fred M. Gottheil, "Increasing Misery of the Proletariat: An Analysis of Marx's Wage and Employment Theory," *Canadian Journal of Economics and Political Science*, 28 (1962): 103–13, and *Marx's Economic Predictions* (Evanston, Ill., 1966), 151–61; M. C. Howard and J. E. King, *The Political Economy of Marx* (Burnt Mill, Harlow, Essex, 1972), 132–35, 199–203; and Walter Eltis, *The Classical Theory of Economic Growth* (London, 1984), chap. 8. There is evidence for both views. Unfortunately, revisionists have failed to take into consideration statements such as Engels made in a note to the 1885 edition of *The Poverty of Philosophy* that "in reality wages have a constant tendency to approach the minimum"; *CW*, 6: 125 n. But, for an attempt to give a systematic account of Marx's theory of wages, see Rowthorn, *Capitalism, Conflict, and Inflation*, 182–230.

With a single qualification, one might argue that John Maynard Keynes made the most fitting commentary on the relations between Malthus and Marxism when he wrote that Malthus and Ricardo provided “an immensely powerful foundation to justify the *status quo*, to ward off experiments, to damp enthusiasm, and to keep us all in order; and it was a just recompense that they should have thrown up Karl Marx as their misbegotten progeny.” The qualification is that Marxism was a legitimate not a “misbegotten progeny.”⁹³

⁹³ Keynes, *The Collected Writings of John Maynard Keynes*, volume 10: *Essays in Biography* (London, 1972), 104–05.

Missionary Preachers in Spain: Teaching Social Virtue in the Eighteenth Century

CHARLES C. NOEL

THE "AGE OF REASON" was more than the epoch of the philosophes. It also witnessed the migrations of the Moravian Brethren, the rise of Methodism, the convulsionists of Saint-Médard, and German Pietism, and in many regions (both Catholic and Protestant) the preaching of itinerant millenarians, prophets, and missionaries. While the philosophes with their rationalist aims directly affected a minority of educated people, popular preachers, like George Whitefield, John Wesley, and the Spaniards studied here, stirred crowds of tens of thousands. Most historians of eighteenth-century culture and society, even though they are well aware of the significance of popular religion, tend to neglect its preachers. Of no country is this truer than of Spain.¹

This essay focuses on the preaching of four leading missionaries of eighteenth-century Spain—Pedro de Calatayud, Antonio Garcés, Diego José de Cádiz, and Miguel de Santander.² Of interest is the social vision of these preachers, the

Earlier versions of this essay were presented at the Institute of Historical Research, London University, to seminars at the Universities of Toronto and Reading, and to the Social Science Research Group, Ealing College of Higher Education. I would like to thank Nicholas Phillipson, Olwen Hufton, Isabel de Madariaga, and William J. Callahan for their support and criticism and Barry Davis and Nicholas Reeves for their encouragement.

¹ The works of Joël Sagnieux and Antonio Mestre Sanchis provide the beginnings of a careful analysis, though with limited attention to the social context in which preaching took place. See, in particular, Sagnieux, *Les jansénistes et le renouveau de la prédication dans l'Espagne de la seconde moitié du XVIII^e siècle* (Lyon, 1976); and Mestre Sanchis, "La reforma de la predicación en el siglo XVIII (A propósito de un tratado de Bolifón)," *Anales Valencinos*, vol. 2, no. 3 (1976): 79–119. William J. Callahan has provided an invaluable survey of the church under the Old Regime, including an excellent analysis of the main challenges facing the church and, briefly, the role of missionary preachers. See his *Church, Politics, and Society in Spain, 1750–1874* (Cambridge, Mass., 1984), chaps. 1–3, esp. pp. 63–65.

² Pedro de Calatayud (1689–1773), a Navarrese Jesuit, studied civil law, philosophy, and theology at several universities and later trained for missionary work at the Jesuit school in Salamanca. A fervent promoter of the devotion of the Sacred Heart of Jesus, he founded congregations dedicated to its encouragement. He conducted missions for over forty years until expelled from Spain with all other Jesuits in 1767. He died in Italy. Antonio Garcés (1701–73), an Aragonese Dominican, held administrative and teaching posts in his order and, like many Dominicans, was a devotee of the rosary. Both Ferdinand VI and Charles III made him a court preacher. Diego José de Cádiz (1743–1801), described by Mestre Sanchis as "the great missionary of the age," was a Capuchin from a humble Andalusian family. A poor student, he fled the intellectual demands of the Dominicans as an adolescent. His sermons and numerous and varied published works made him enormously popular. His letters to his spiritual advisers reveal him as an ascetic who had a deeply spiritual and unsophisticated sincerity. See Ricardo García Villoslada, *Historia de la iglesia en España*, volume 4: *La iglesia en la España de los siglos XVII y XVIII*, ed. Antonio Mestre Sanchis (Madrid, 1979), 628. Miguel de Santander (1744–1831), a Capuchin, is the only one of these preachers who could be considered a proponent of

manner in which they communicated that vision to society, and the reaction they aroused. Their sermons reveal the values of religion, community, social relationships, and business life that these men expressed to their audiences not only in spoken but also in published form. From them we may understand the dynamic relationship the preachers often had with the crowds who gathered to hear them. The preachers upheld, sometimes only implicitly, the traditional assumptions of Spain's Christian, overwhelmingly agrarian, and hierarchical society. With the exception, in part, of Miguel de Santander, their lessons and warnings reinforced traditional values against what they believed were the disruptive effects of the economic and social changes Spain began to experience in the eighteenth century. They feared that these changes—commercial, industrial, and financial—would have a baneful effect on urban and rural communities. Flourishing commerce would encourage greed, injustice, and immorality. The integrity of Christianity would be threatened, the church weakened, Christian virtue destroyed, agrarian society undone. In their appeal against change the preachers captured the support particularly of the rural masses, agrarian elite, and urban poor. The context of their sermons helps explain their popular support as well as the hostility they aroused among enlightened reformers.

French historians have cast considerable light on French missionary preachers of the seventeenth and, to a lesser extent, eighteenth centuries.³ Because of their work we understand much about the financing and organization of French missions, the ecclesiastical and popular milieux in which they were conducted, and the nature and content of sermons. Hence, the French precedent can often assist our understanding of Spain insofar as the different circumstances of Spanish society and politics allow. Spanish Catholicism did not experience the same degree of erosion by secularization that weakened French Catholicism in the eighteenth century, and Spanish preachers enjoyed a more favorable reception than their French counterparts. Nevertheless, the French example can contribute to an analysis of the Spanish experience as derived from a study of missionary sermons. The published versions of these sermons probably do not differ significantly from

enlightened attitudes. He was also the only one to enter the hierarchy of the secular church. An extremely active and charitable preacher, he accepted the see of Huesca from Joseph Bonaparte during the Napoleonic interlude. The collapse of Spanish Bonapartism forced him into exile until 1820. He was a prolific author of spiritual works and an influential innovator of Spanish preaching in the 1780s and 1790s. Diego José, a life-long friend, greatly admired his effectiveness as a missionary. On Pedro de Calatayud, Diego José de Cádiz, and Miguel de Santander, see Sagnieux, *Les jansénistes et le renouveau de la prédication*, 139–43, 320–30; and Antonio Elorza, "Cristianismo ilustrado y reforma política en fray Miguel de Santander," *Cuadernos hispanoamericanos*, 214 (1967): 73–107.

³ See, especially, Louis Pérouas, "Essai sur l'histoire des missions à l'intérieur de la France," in J. F. Kotte, ed., *La mission générale* (Paris, 1961), and *Le diocèse de La Rochelle de 1648 à 1724: Sociologie et pastorale* (Paris, 1964). Pérouas's *Le diocèse de La Rochelle* is less suggestive on missions. Also see Marc Venard, "Les missions des oratoriens d'Avignon au XVII^e et XVIII^e siècles," *Revue d'histoire de l'église de France*, vol. 48, no. 145 (1962): 16–38; Jeanne Ferté, *La vie religieuse dans les campagnes parisiennes (1622–1695)* (Paris, 1962), esp. chap. 3; and Charles Berthelot du Chesnay, *Les missions de Saint Jean Eudes: Contribution à l'histoire des missions en France* (Paris, 1967).

the original oral texts.⁴ Private letters of some of the preachers, a small number of hagiographical studies, and a few state papers give additional evidence. No claim is made that the four preachers chosen were typical of the large number of missionaries performing at many levels of competence and effectiveness in their time. The primary reason for choosing them is the extraordinary popularity they attained. Along with a few others, including Nicolás Gallo and Francisco de Villalpando, they were perhaps the most influential preachers of the century.⁵

That the most successful missionaries gained enormous popularity is well known to historians of this period. Antonio Domínguez Ortiz wrote that the most popular clerical figures of the time in Spain were the charitable bishop and the missionary friar. Because of the missionaries' efforts, according to Antonio Mestre Sanchis, "the missionary movement achieved an extraordinary success in the age of enlightenment." William J. Callahan declared that the missionary movement became "the most vigorous spiritual effort of the eighteenth-century church," sending preachers "across the length and breadth of the kingdom."⁶ Although these preachers appealed primarily to the rural masses, agrarian elite, and urban poor (including artisans), urban aristocrats and professional men—for example, notaries, lawyers, and doctors—attended their sermons and lent support. Miracles were often attributed to them. The dead body of Garcés was seized by a frenzied crowd of relic hunters who stripped it in their excitement. Once, years before at a time of high bread prices, a noisy crowd besieged Garcés in his monastery, demanding that he intercede with municipal authorities on behalf of the poor. Garcés in 1766, Diego José de Cádiz in 1778, and the Capuchin missionaries of Barcelona during the riots of 1789 showed that some preachers were able to sway large and violent crowds. The Capuchin friars calmed the rebels, converted a riot into a religious procession, and finally persuaded the mob to return home.⁷ The preachers were unique in the love and trust that they inspired among Spaniards, except for the enlightened reformers, who grew in number and influence in the late eighteenth century. The reformers were intellectuals, lawyers, bureaucrats, and powerful ministers under Charles III (1759–88) and Charles IV (1788–1808). Normally they were hostile to church authority and clerical independence and distrusted and feared the missionary preachers who had become the great pillars of Catholicism. Enlightened ministers, such as Pedro Rodríguez Campomanes, Pablo de Olavide, the count of Floridablanca, and the count of Aranda, treated the preachers contemptuously and used the authority of government or Inquisition

⁴ Hilary Dansey Smith has provided a mild caveat. See her *Preaching in the Spanish Golden Age: A Study of Some Preachers of the Reign of Philip III* (Oxford, 1978), viii. Many studies of the Spanish missionary movement have concentrated on missions in Spain's overseas empire.

⁵ Gallo's style of oratory was popular and influential; Villalpando, a Capuchin, was a well-known missionary.

⁶ William J. Callahan, "The Spanish Church," in William J. Callahan and David Higgs, eds., *Church and Society in Catholic Europe of the Eighteenth Century* (Cambridge, 1979), 43. Mestre Sanchis also pointed out the opinion of Domínguez Ortiz; *La iglesia en la España de los siglos XVII y XVIII*, 627.

⁷ Sebastián Espallargas, *Vida del v. padre maestro fr. Antonio Garcés* (Madrid, 1788), 183–90; and Basili de Rubí, "Los capuchinos en la revolución de Barcelona de 1789," *Estudios Franciscanos*, vol. 69, no. 333 (1968): 337–54.

to intimidate, humiliate, and punish three of the four men studied here. The reformers confronted preachers who were politically perceptive and intensely sensitive to social problems.

ACCORDING TO THE FRENCH HISTORIAN Jean Delumeau and to others who share his interpretation, the Catholic church of the seventeenth and eighteenth centuries still faced the great challenge of Christianizing the masses of nominally Catholic Western Europe. In Spain, as elsewhere, the eighteenth-century church continued its efforts to impose post-Tridentine beliefs and practices on communities that appeared mired in ignorance and superstition. Spanish and other European missionaries had to cope with the onslaught of rationalism and anticlericalism, even while struggling to overcome medieval religiosity. The result in Spain was an intense and widespread missionary effort to reconquer lost souls and implant the banner of reformed Catholicism. Inspired by the examples of St. Vincent de Paul in France and St. Alphonsus de Liguori in Italy, Spanish preachers led a far-reaching movement to impart the basics of religion to the masses in cities, towns, and villages.⁸ Like the French preachers, they often employed the vocabulary and images of the battlefield. They were soldiers leading the attack on the devil's fortress. Europe "hummed with travelling missionaries," summoning the faithful to fight God's cause against sin.⁹

In Spain almost all preachers belonged to a religious order, most frequently Capuchin or Jesuit. Some orders, such as the Capuchins and the Spanish Oratorians of the Savior, were organized primarily to preach missions, while others, like the Dominicans, only occasionally supplied a brother to the movement. The missionaries preached in almost any suitable venue—not only in churches, chapels, hospitals, and other public buildings but also in the streets, squares, jails, and even galleys. Although medieval preachers had worked in the cities, the post-Tridentine missionaries also went into the villages and countryside.¹⁰ They normally worked in pairs, a principal with his companion, sharing the burdens of travel (often on foot), organization, and sacramental duties. Some confined their preaching to one or two provinces, while others traveled widely. Their educational and intellectual accomplishments were varied. Some, like Pedro de Calatayud and Miguel de Santander, were men of learning and culture. Others, like Diego José de Cádiz, eschewed learning and rejected intellectual pursuits.¹¹ Whatever their

⁸ Delumeau, *Catholicism between Luther and Voltaire: A New View of the Counter Reformation*, trans. J. Moisionier (London, 1977), esp. pt. 2: pp. 189–90 and chap. 5; Mestre Sanchis, *La iglesia en la España de los siglos XVII y XVIII*, 627–29; Alfredo Martínez Albiach, *Religiosidad hispana y sociedad borbónica* (Burgos, 1969), 402; and Callahan, "The Spanish Church," 47.

⁹ One of the high points of many Spanish missions was the sermon known as the *asalto*, a lively attack on the devil's power. Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 114; and Delumeau, *Catholicism between Luther and Voltaire*, 189. On missions in France, see Berthelot du Chesnay, *Les missions de Saint Jean Eudes*, 66.

¹⁰ Delumeau, *Catholicism between Luther and Voltaire*, 190; and Smith, *Preaching in the Spanish Golden Age*, 16.

¹¹ Venard, "Les missions des oratoriens"; Elorza, "Cristianismo ilustrado"; and Mestre Sanchis, *La iglesia en la España de los siglos XVII y XVIII*, 13–14. For brief discussions of patterns of missionary work, see Martínez Albiach, *Religiosidad hispana y sociedad borbónica*, 2, 403–04. For a biographical sketch, see Juan Fernández-

intellectual achievements, their primary aim was to fill lacunae left by parish priests. Ordinarily, the latter were indifferent and infrequent preachers, and their sermons did not satisfy the parishioners' hunger for spiritual excitement. Often they had failed to teach their flocks the basic elements of Catholic doctrine and let the missionaries assume this duty. Sometimes social and financial relationships between parishioners and priests had strained spiritual ties. And many communicants undoubtedly preferred, as they still do, to confess occasionally to an outsider, a priest whose disapproval might be more professional and easier to bear than that of the parish priest. Like his modern counterpart, the missionary had an opportunity to stir the souls of Catholics in a way the ordinary priest could not—to induce, in Delumeau's words, a sense of religious shock. Once the missionary departed, the task of the parish priest was to transform this shock into a durable spiritual life.¹²

The missionaries aimed to leave audiences "contented with God and religion and discontented with themselves," causing a pain in the heart that would insure that their sermons were not forgotten.¹³ Garcés wanted "to implant a horror of sin, to convert hearts to the Lord, to reform habits, and to establish virtue among people of all conditions."¹⁴ The key to conversion was a deep, sincere penitence of a kind seldom demanded by ordinary confessors. As Garcés suggested, parishioners were often misled by indulgent priests to believe penitence was easy. "Many spend their lives, or a great part of them, sinning and confessing, confessing and sinning; they do no more than confess their guilt and then return to their accustomed wretchedness; they are like the buckets of wells, they ceaselessly descend and rise, first emptying their water and then refilling."¹⁵ Spanish preachers followed St. Vincent de Paul in focusing their missions on general confessions, during which the participants manifested penitence. Confession justified the entire exercise, to which processions, sermons, lights, pictures, handbills, and other paraphernalia were inducements.¹⁶ The preachers divided their day between pulpit and confessional and deemphasized formal teaching and catechizing in favor of the sermon's emotional impact. They preached not only about the private, personal Christian virtues but also about social virtues manifested in the family and, especially, in the economic and political community. For them, the soul was not an abstraction existing only within the heart but a real entity; salvation had to be achieved in a social context—in a village or town and among neighbors and family. Christian virtues had to be practiced in the

Anchuela y Collado, ed., *Tres doctrinas prácticas, íntegras del venerable padre Pedro de Calatayud y Florencia*, S.I. (Madrid, 1951).

¹² Delumeau, *Catholicism between Luther and Voltaire*, 194.

¹³ "Sermones del p. fr. Alonso de Cabrera," in Miguel Mir, ed., *Predicadores de los siglos XVI y XVII*, 1 (Madrid, 1906), xii.

¹⁴ Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 41.

¹⁵ *Ibid.*, 94, 60.

¹⁶ Delumeau, *Catholicism between Luther and Voltaire*, 193; Pérouas, "Essai sur l'histoire des missions," 44–50; Berthelot du Chesnay, *Les missions de Saint Jean Eudes*, 36; and Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 37, 93.

marketplace and the law court as well as in the confessional.¹⁷ The preachers shared the Aristotelian understanding of man as, above all, a social and political being. In the words of Miguel de Santander, "God, our Master, has created you for society; you live in the midst of your community [*pueblo*]."¹⁸ The missionary was not only a superpriest but also an itinerant teacher of Christian social virtue.

The preachers measured their success by the number of general confessions, the hours spent in the pulpit, the size of crowds, praise from the local clergy, and the attendance and support of the local elites. A prebendary of Cartagena cathedral, though not himself a preacher, catalogued the effects of one of Diego José's missions in Murcia. The preacher had reconciled enemies, reunited alienated couples, encouraged parents to treat their children more lovingly, promoted deep, tearful penitence, heard many general confessions, brought large numbers to church worship, encouraged more frequent communion, and promoted "a great reform of customs," discouraging luxury, blasphemy, and other offenses.¹⁹ In 1752 Pedro de Calatayud had to preach in the open air, because the crowds were far too great even for the large cathedral of Toledo, while Diego José estimated some fifty thousand heard him in the palace square in Barcelona. Many similar claims were made and, though possibly exaggerated, were often supported by more impartial evidence. Diego José, at least, was frank enough also to record his failures. In the small Andalusian towns of Estepa and Fuentes, for example, he had to give up and withdrew after two weeks of vain effort.²⁰ Indifference was not rare—a point noted by French historians.²¹ In any case success was normally short-lived. Transgressions declined after a mission, but, as one French curé noted flatly, "they always reappeared."²² For this reason, missionaries often returned to a region or town repeatedly. Diego José preached at least six missions in Málaga.

The task of organizing and financing a mission could be a considerable burden. In comparison to historians' knowledge of financial support for French missions, little is known about the endowments supporting missions in Spain.²³ Diego José and Garcés kept their expenses down, and the latter refused to permit any town where he preached to spend anything on him. Diego José saved by walking from

¹⁷ The preachers' manuals apparently encouraged this idea of social virtue. Diego de Estella, the sixteenth-century Franciscan preacher whose manual for missionaries was often reprinted in the seventeenth and eighteenth centuries in Spain and elsewhere, advised preachers to start missions by gaining the community's confidence. Then, he wrote, missionaries should chastise the community in general terms and, finally, condemn the specific offenses known to be perpetrated in the locality. Diego de Estella, *Modo de predicar y modus concionandi*, ed., P. Sagüés Azcona, 2 vols. (Madrid, 1951), 2: 83–84.

¹⁸ Miguel de Santander, "Sobre la devoción," in his *Doctrinas y sermones para misión*, 6 vols. (Madrid, 1800), vol. 2. Pedro de Calatayud also made it clear that living a virtuous life in one's community was part of the means of salvation. Pedro de Calatayud, *Misiones y sermones del padre Pedro Calatayud*, 2 vols. (Madrid, 1754), 2: 9.

¹⁹ Antonio Rovira y Galván, as quoted in Serafín de Hardales, *El misionero capuchino: Compendio histórico de la vida del venerable . . . fr. Diego José de Cádiz* (Cádiz, 1811), 42.

²⁰ Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 67; and Joaquín Torres Asensio, *Historia de la vida interior y exterior del bienaventurado fray Diego José de Cádiz*, ed., J. J. Alcober Higuera (Madrid, 1894), 52, 172.

²¹ See, especially, Ferté, *La vie religieuse*, 213; and Berthelot du Chesnay, *Les missions de Saint Jean Eudes*, 109.

²² Venard, "Les missions des oratoriens," 32. For similar conclusions concerning present-day Latin America, see Emile Pin, *Elementos para una sociología del catolicismo latinoamericano* (Fribourg, 1963), 19.

²³ On France, see Venard, "Les missions des oratoriens"; and Berthelot du Chesnay, *Les missions de Saint Jean Eudes*. On Spain, see Smith, *Preaching in the Spanish Golden Age*, 16–17.

mission to mission—sometimes over vast distances—even when sick. Both normally depended on free hospitality offered by clerical or lay supporters, but they could not always obtain it. If the mission was not endowed, money had to be raised not only for lodging and food but also for transport, publicity, handbills, candles for processions, rosaries, and images carried by the preachers or distributed among the people. The most prominent preachers could expect to cover the costs from various charities. Less reputable missionaries expected payment for their services; the most skillful collected large gifts of money, food, and even clothing and jewelery, all thinly disguised as charity. The greediest and most brazen even threatened, after arousing expectations of a memorable pulpit performance, not to preach if they were not paid in cash. But the four eminent preachers studied here avoided any hint of the mercenary. We know that Garcés gave away unneeded stipends, and the others probably did so as well.²⁴ An episcopal license was required of missionaries before preaching in a particular diocese, and the four were careful to obtain it. Garcés would not go on a mission unless invited, often by the bishop himself. Consequently, missionaries cultivated prelates, sometimes accepted invitations to dine and lodge with them, and received their offers of admiration. In 1782, for instance, the cultivated and enlightened Francisco Antonio de Lorenzana, archbishop of Toledo, greeted the arriving Diego José outside Toledo's city walls, dismounted from his coach, and escorted the preacher on foot to the latter's lodging in a Capuchin monastery. The prelates were rewarded for their support and flattery when preachers directed their missions into channels that met particular diocesan needs. Prominent laymen sometimes called the preachers in on their own initiative or in conjunction with clerics. Thus, Diego José had his debut at court when the pious duke of Medinaceli invited him to preach at Aranjuez, with Charles III's permission, and Garcés was invited to Pamplona by both the viceroy of Navarre and the bishop.²⁵ The preachers cultivated their supporters among the powerful and rich, who could provide access, sustenance, and protection.

Preachers differed to some extent in their methods of organizing and conducting missions: some depended heavily on a companion; some employed showy effects; others emphasized simplicity and directness. Oratorical styles differed among them and evolved considerably during the century. Yet their missions contained the same basic elements. General sermons were delivered to the entire community; others, to groups with special occupational or spiritual interests—priests, nuns, lawyers, notaries, and the like—usually in the larger towns and cities; confessions were heard and penance given; processions were organized and led; and special ceremonies were organized to inaugurate and close the mission. The preachers sometimes called on local volunteers to aid them in minor tasks, for example, asking youngsters to help set up benches in the hall or church.

²⁴ Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 82, 166–67; and Pedro de Calatayud, *Misiones y sermones*, 1: 27–28.

²⁵ Torres Asensio, *Historia de la vida*, 112, 115–16, 170; Hardales, *El misionero capuchino*, 46; and Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 98.

Occasionally local circumstances—bad weather or an epidemic—lent to an entire mission a special character. Then the mission became an exercise in community prayer and probably in the reaffirmation of community solidarity at a time of distress and tension.²⁶ The processions through the streets of village or town were a vital element in most missions, exciting the crowds that gathered to watch or participate. Missions usually opened with a procession, frequently staged at night with an abundance of lanterns. Such spectacles were common (in France as well as in Spain). Processions helped publicize the mission, and a large procession magnified the preacher's authority.²⁷ In his processions, Pedro de Calatayud, armed with a crucifix and accompanied by an assistant ringing a little bell, pronounced a series of *sentencias*—terrifying warnings of the sinner's fate based on verses from Job, Jeremiah, and other scriptures. He directed most of his warnings at the rich, powerful, and aristocratic, repeatedly reminding them of the difficulty the rich had in achieving salvation.²⁸ Garcés preferred a calmer procession and initiated his missions with the saying of the rosary, a devotion of which he was a leading propagandist, gathering the crowds in a double column as he passed through the streets. He continued with the opening sermon, during which he displayed his favorite image of the Madonna, encircled with roses, bearing the infant Christ. Holding it above the crowd, he began the first act of contrition. Thereafter, he delivered dawn sermons (the *misa de la aurora*), preceded by daily processions, timed to attract peasants and artisans before work.²⁹ Less scrupulous preachers stooped to side-show trickery to spark interest. Pedro de Calatayud told of a fellow Jesuit who sweetly played the harp to soften up his audience for the sermon and of other preachers who "put their naked arms in the flame of a torch to inspire terror of the pains and fires of hell."³⁰

Once launched, a mission might last from a few days to a month or more, depending on the size of the community. The latter included the largest administrative, commercial, and shipping centers—Madrid, Barcelona, Bilbao, Málaga; important medium-sized towns, such as Jérez, Alicante, Sanlúcar, and Salamanca; smaller cathedral cities, such as Cuenca and Zamora; and small towns and villages, such as the *lugarcito* of Jusliból, visited by Garcés, and Arjonilla, visited by Diego José.³¹ The four missionaries seem normally to have spent a week to ten

²⁶ Diego José de Cádiz, *Cartas íntimas del beato Diego José de Cádiz dirigidas al p. fray Eusebio de Sevilla*, ed. Diego de Valencina (Cádiz, 1943), 69; and A. N. Galpern, *The Religions of the People in Sixteenth-Century Champagne* (Cambridge, Mass., 1976), 85–86.

²⁷ Venard has suggested that all of the missionaries he studied used spectacular ceremonies to awaken interest ("Pas de mission sans quelque cérémonie plus spectaculaire"); "Les missions des oratoriens," 28. Torchlight processions are still popular in Latin America, and enormous crowds gather to hear sermons; Pin, *Elementos para una sociología*, 27.

²⁸ For examples, see Pedro de Calatayud, *Misiones y sermones*, 1: 342; and Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 44.

²⁹ Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 83–86.

³⁰ Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 28.

³¹ Historians have questioned the frequency with which the missionaries preached in villages and small towns. The sources, particularly in the cases of Garcés and Diego José, mention many of the localities they visited, but it is almost impossible to ascertain the size of the communities in which they preached. Nevertheless, the preachers and their biographers indicate that missions were held in a large number of towns that even in the late nineteenth century had populations of only a few thousand. Since they frequently walked

days on missions conducted in towns and cities. At some point (early in the mission, Pedro de Calatayud advised), the preachers visited the sick at the hospital, praying for their recovery, and prisoners in jail, urging them to repent and confess. Then, before the end of the mission, they urged the entire community to share their charity with both the lawbreakers and the ill. Pedro de Calatayud suggested that food and wine be collected for the sick and imprisoned, to be delivered by a procession—consisting of the local clergy, law officers, nobles, and other ranking citizens led by the preacher—as a means of reinforcing charitable virtues and heightening, undoubtedly, the authority of the clergy and other local hierarchs. In the processions of penitence, participants walked, appropriately robed, accompanied by chanting clergy, choirboys, political chieftains, and other supporters.³²

During the early modern era fashions of pulpit oratory oscillated in Spain. In the sixteenth century, preaching was usually direct, simple, and effective—a “popular Christian preaching” that evoked deep emotion and piety among the uneducated.³³ By the first decades of the succeeding century preachers had begun to affect a different style. Influenced by the cleric and poet Luis de Góngora y Argote and his friend Hortensio Paravicino, an “opulent and immoderate” court preacher, most priests, whether missionaries or not, rejected what now appeared to be demeaning vulgarisms of popular language and simplicity in favor of inflated, elaborate, overcultivated Gongorism in the pulpit.³⁴ They adopted novel images, sought to surprise, and used complicated literary allusions. They gloried in paradoxes, allegories, inappropriate metaphors, and intricate parallelisms, even introducing pagan themes so that they might, for example, threaten their audiences with the anger of an Aztec rain god. These fashionable sermons often lacked moral content. Worse, they were almost unintelligible to most listeners, wasted on the uneducated who eventually rejected both the message and the messenger.

The middle third of the eighteenth century witnessed an inevitable reaction against baroque excesses. Decades of criticism by scholars, such as Gregorio Mayáns y Siscar, by a number of Jesuit reformers, and later in the century by Jansenist prelates, such as Archbishop Armañá of Tarragona, had a significant effect. Early criticism culminated in the satirical figure of the preacher, Fray Gerundio, created by the Jesuit novelist José Francisco de Isla. In his novel,

from one mission to the next, they must have spent much time on country roads passing through and becoming familiar with small villages. We occasionally get some indication of the wide net they cast; for example, Garcés's biographer wrote that the missionary preached “in all those [places] of the district [of Leciñena—itsself an insignificant town or village in Aragon]” in 1742. In any case, William J. Callahan was rightly confident enough to state, referring to Pedro de Calatayud's career, that he spent decades “preaching in the small towns and remote villages of central and northern Spain”; “The Spanish Church,” 47. For his discussion of missions in the years 1742, 1749, 1756, and after, see Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 74, 170.

³² Pedro de Calatayud, *Misiones y sermones*, 1: 164–65, 321–26.

³³ Mir, *Predicadores de los siglos XVI y XVII*, vi, v–xvi; and Pedro Antonio Sánchez, *Discurso sobre la eloquencia sagrada en España* (Madrid, 1778), 65–71.

³⁴ Antonio Domínguez Ortiz, *The Golden Age of Spain, 1516–1659* (London, 1971), 257.

perhaps the most popular literary work of the century, Isla wrote, "The preacher must teach in a clear, sharp style, intelligible to all, adapted to ordinary ideas so that it will be equally understood by the plebeian and the aristocrat, the man of culture and the rustic, . . . the wise and the ignorant, . . . and, in a word, he must persuade and move all." In the late eighteenth century clear and appropriate language became coupled with a widespread tendency to rely heavily on rational argument rather than biblical precept and to allow what one historian has ambiguously called "the human element" to acquire an "importance it had not had earlier."³⁵

As straightforward as their style had become, the greatest preachers still used innocent tricks to delight the crowd and to win their attention. They employed irony, told jokes and dramatic stories, and recited short, snappy poems. Sometimes they were corny. Yet they poured great energy into their performances, conveying their enthusiasm, using their entire bodies to do so. They exhausted themselves in the pulpit in the attempt to "grasp the innards of the listeners." Diego José's sermons cost him "a thousand pains and drops of blood and even the words, as with draghooks, are wrenched from my entrails."³⁶ Their sincerity impressed both the illiterate and the cultivated. A Seville cleric praised Diego José's energetic expression, persuasiveness, and "the movements of his face, body, and hands, natural and in themselves eloquent."³⁷ The preachers boasted of the appeal they had for people on all social levels, and they or their biographers described the favorable response of the entire community, even the most uneducated peasants. Such reports have to be accepted with caution, but there is much evidence to show that the preachers succeeded in their attempt to build a rapport with most social groups. Alfredo Martínez Albiach, a historian of eighteenth-century Spanish religiosity, wrote, "They had as their normal audience the entire community [*el pueblo entero*], without distinction of class, gathered together in the largest church or in the square . . . [and] the missionary practiced an oratory rooted in the ordinary people, leveling [*niveladora*], accessible to all."³⁸

In 1787 a prebendary of Cartagena cathedral reported at length (and with admirable numerical precision) on the impact Diego José had during his mission in Murcia.

Murcia had wanted for many years to hear this apostle and had requested him to come many times. . . . On the arrival of this humble Capuchin, the city found itself already inundated by the arrival of countless outsiders who pressed forward to hear and see him. Never had such a commotion been witnessed in Murcia. From towns twelve and fifteen leagues distant

³⁵ Isla, *Fray Gerundio de Campazas*, ed. Russell Seebold, 3 (Madrid, 1960), 140; F. González Olmedo, "Restauración de la oratoria sagrada en el siglo XVIII," *Razón y Fe*, 51 (1918): 469, and "Decadencia de la oratoria sagrada en el siglo XVII," *Razón y Fe*, 46 (1916): 310–21; Smith, *Preaching in the Spanish Golden Age*; Mestre Sanchis, *La iglesia de la España de los siglos XVII y XVIII*, 630–31, and "La reforma de la predicación en el siglo XVIII," 79–96; and Saugnieux, *Les jansénistes et le renouveau de la prédication*. Also see Seebold's introduction to the 1960 edition of Isla's *Fray Gerundio de Campazas*.

³⁶ Francisco Terrones del Caño, as quoted in Mir, *Predicadores de los siglos XVI y XVII*, xvi; and Diego José, as quoted in Torres Asensio, *Historia de la vida*, 83.

³⁷ Torres Asensio, *Historia de la vida*, 41–42.

³⁸ Martínez Albiach, *Religiosidad hispana*, 405.

the number of persons who came with only this purpose was very great. . . . And they totaled 29,540. In the processions of the rosary many were barefoot, chanting and singing with music the Ave Maria and the Santísima Trinidad recommended by Father Cádiz. In some processions there were 252 torches and 392 wax candles, and generally the animated light of a devout and Christian dedication shone among all who were there.

The prebendary praised Diego José's preaching, which made his sermons comprehensible and memorable to even the least educated, and described the extensive charity with which the mission became associated. The bishop, aided by the local clergy, doled out bread and cash to thousands of poor people who had flocked into Murcia for the occasion. The marquis of Veniel also gave cash to the peasants from his estates.³⁹ Thus, the eleven days of the Murcia mission were transformed into an event of social as well as spiritual significance.

THE POWER OF ORATORICAL SKILL notwithstanding, the message itself was what gave the sermons their immediacy, and here we find the best explanation of the preachers' popularity. In the seventeenth and early eighteenth centuries, French clerics, both curés and missionaries, manifested in their sermons a special concern about the sins of sexuality and economic injustice, frequently denouncing usury, theft committed by the powerful against the poor, cheating on contracts, excessive corvées, and similar infractions. Furthermore, some clerics, such as Bishop Pavillon of Alet, used their sermons and instructions to protest the exploitation of peasants.⁴⁰ Spanish missionaries dealt to a remarkable degree with similar offenses, showing in their sermons a virtual obsession with sexual morality and economic virtues. Like Garcés, who believed that "lewdness [*luxuria*] was the mother of nearly all the heresies" and certainly of numerous sins, they espoused puritanical standards, disapproving, particularly, all physical indulgence. In addition, they feared the impact of enlightened thought, a phenomenon they attributed to libertinism. Pedro de Calatayud preached against "that false philosophy by which the enchanted are deceived," and during most of his career Diego José campaigned against unbelief. During a mission in Granada he delighted priests and prelates by denouncing enlightened ideas at the university and in the cathedral.⁴¹

And yet the struggle against the Enlightenment was of less significance to the preachers than that against sexuality and social and economic injustice. They denounced mixed dancing, even innocent games, free conversation between youths and young girls, and almost any activity that brought the sexes together without the strictest supervision. They attacked the theater with special vehemence and frequency, and all four, including the generally moderate Miguel de Santander, singled out playhouses and, in particular, the farces performed in them

³⁹ Antonio Rovira y Galván, as quoted in Hardales, *El misionero capuchino*, 29.

⁴⁰ This idea was suggested by Robin Briggs in a paper presented to the Seminar on European History, 1650–1800, at the Institute of Historical Research, London University, in 1978.

⁴¹ Garcés, as quoted in Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 95; Pedro de Calatayud, as quoted in Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 69; Torres Asensio, *Historia de la vida*, 62–64; and Mestre Sanchis, *La iglesia en la España de los siglos XVII y XVIII*, 14.

as sinful. In a tradition dating from at least the early seventeenth century, the preachers manifested their fear of the theater, the frivolities or worse of theatergoers, and probably as well their jealousy of the theater's popularity and its capacity to compete for audiences. In Madrid, Pedro de Calatayud's warnings against playgoing so affected his audiences that they persuaded the king to have the playhouse shut (for a time). Diego José had a similar effect in Córdoba and Jaén. The municipal government of the latter was driven to such a pitch of repentance that it planned to prohibit permanently the performance of comedies, opera, and other public theatricals. Most effective of all were the sermons of Garcés in Burgos. There, the city fathers first closed the theater and then, unsatisfied, had it torn down. As the preacher's biographer wrote, "What was once a school of iniquity today serves as a cattlepen."⁴² At the same time the preachers expressed, even with regard to the theater, a surprising social concern. Pedro de Calatayud urged that his hearers, if they continued to risk their souls by attending performances, at least not smoke before or during the performance, "because of the risk of fire and the offense caused to others by the smoke and odor."⁴³ More constructively, in Fuentes Diego José suggested that to build and support a playhouse offended God, partly because the funds would be better spent on a hospital, a girls' asylum, and barracks for soldiers.⁴⁴

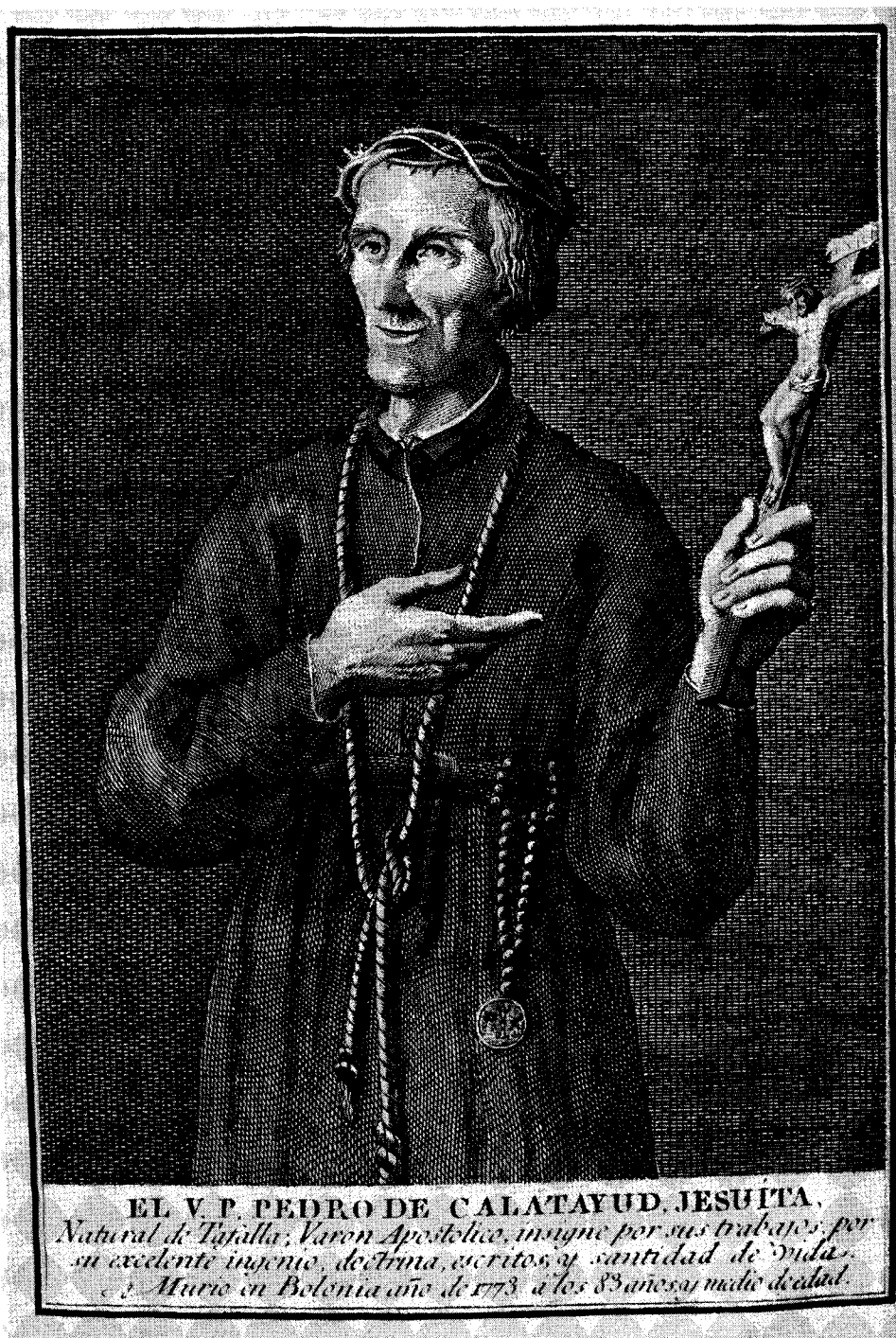
The missionaries also condemned idleness, sharing with leading enlightened reformers, such as Campomanes and the journalist Luis Cañuelo, a repugnance toward idleness among both rich and poor. But whereas the enlightened reformers sought to encourage productivity, the preachers wanted to discourage the spiritual decay and temptations to sin they associated with idleness. Not surprisingly, Miguel de Santander, the most sympathetic of the four preachers toward the reformers' campaign for hard work from all, was especially concerned with this issue. In language that neatly combined traditional Christian concepts with the vocabulary of the Enlightenment, he taught that idleness not only contributed to vice and disorder but also injured agriculture and trade. Such an existence "lacked usefulness to society" and to "one's state, one's calling, and one's religion."⁴⁵ In a sermon delivered to the Murcian aristocracy in 1734, Pedro de Calatayud reminded them that the nobility of virtue and spirituality took precedence over noble lineage. Even the ancestry of the highest born would show many "bastards, low-born men, rustics, plebeians, and persons of mean trades." Once achieved, the status of nobility produced self-indulgence, ridiculous eti-

⁴² Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 121; Pedro de Calatayud, *Cathecismo práctico y muy útil para la instrucción y enseñanza fácil de los fieles* (Seville, 8th printing, n.d.), 234, and *Opúsculos y doctrinas que para el gobierno interior...* (Logroño, 1754); Smith, *Preaching in the Spanish Golden Age*, 10; Miguel de Santander, *Doctrinas y sermones para misión*, 2 (2d edn., Madrid, 1802): sermon 9; and Torres Asensio, *Historia de la vida*, 48, 92–93.

⁴³ Pedro de Calatayud, *Opúsculos y doctrinas*, 55.

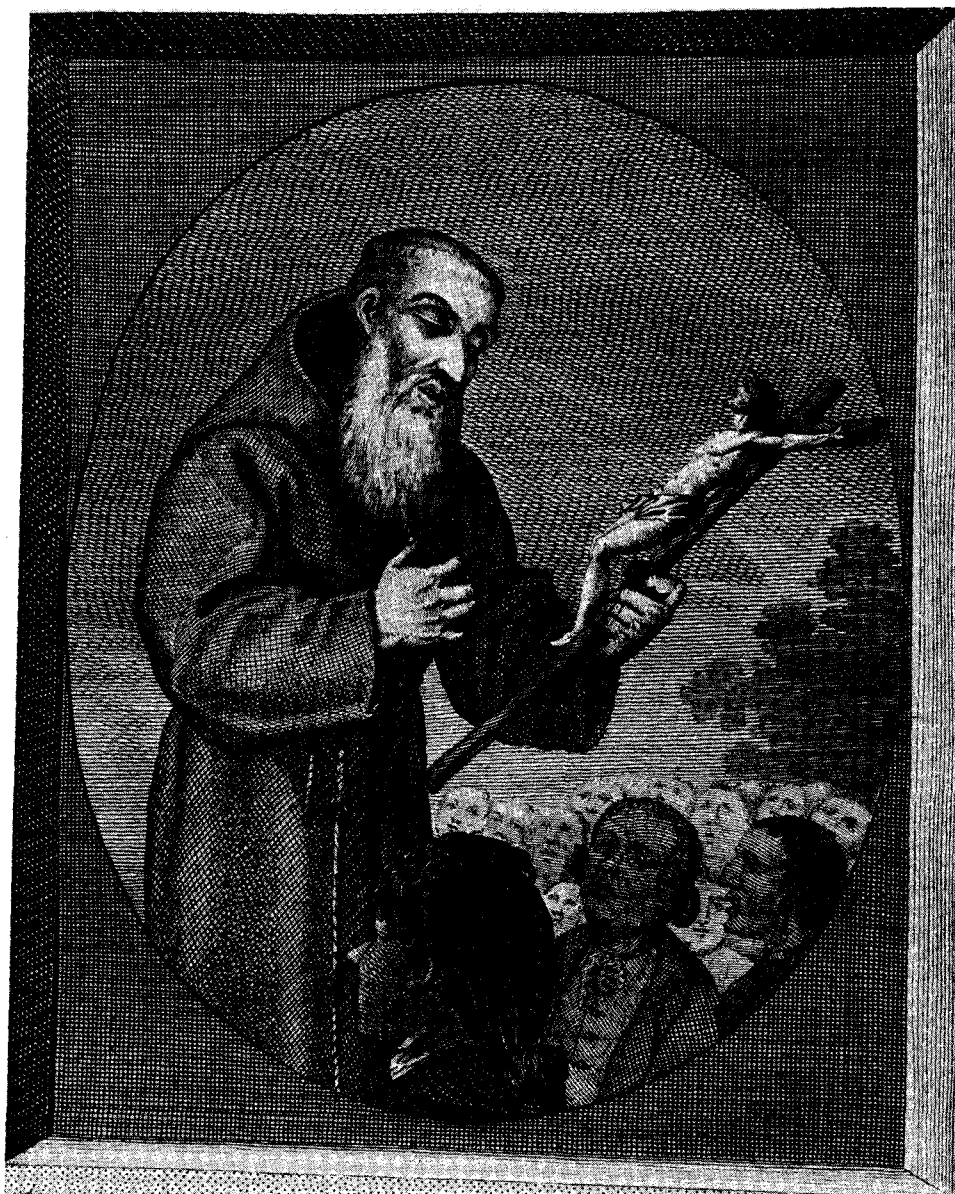
⁴⁴ Torres Asensio, *Historia de la vida*, 58.

⁴⁵ Miguel de Santander, *Pláticas doctrinales para misiones* (rev. edn., Barcelona, 1851), 234–40, and *Doctrinas y sermones para misión*, 3 (3d edn., Madrid, 1808): 340, 391. On Campomanes and Cañuelo, see Richard Herr, *The Eighteenth-Century Revolution in Spain* (Princeton, N.J., 1958), 50–52, 184–85.



EL V. P. PEDRO DE CALATAYUD. JESUITA.
*Natural de Tajalla. Varon Apostolico, insigne por sus trabajos, por
su excelente ingenio, doctrina, escritos y santidad de vida.
Murió en Bolonia año de 1773 a los 83 años y medio de edad.*

The Jesuit preacher and scholar Pedro de Calatayud, portrayed wearing the symbols of penitence, prayer, and Christ's passion. This engraving is the frontispiece of his *Doctrinas prácticas que solía explicar en sus misiones el v. p. Pedro de Calatayud* (Madrid, 1797) and is reproduced courtesy of the Biblioteca Nacional, Madrid.



*1.º R. del Exemplar varon F. Diego Joseph de Cádiz, Misionero, 1.º Cap.
de la Provincia de Andalucía, murió en la Ciudad de Ronda el 24 de Marzo
de 1801 a los 38 años de edad, está depositado en la Iglesia de N.º S. de la Pa.*

An unusual engraving showing the Capuchin Diego José de Cádiz preaching to an audience. Like other preachers, he directed the crowd's attention to the crucifix, encouraging an emotional response to the image of Christ's suffering. Copies of this engraving would have been sold to the public in Madrid and elsewhere. Engraving reproduced courtesy of the Biblioteca Nacional, Madrid.



An elegant portrait of Diego José de Cádiz showing him in rural Galicia during a mission he conducted there in 1795. The engraving appears in *Colección de las obras del r. p. fr. Diego José de Cádiz* (Madrid, 1796) and is reproduced courtesy of the Biblioteca Nacional, Madrid.

quette, pride, presumption, and, worst of all, indolence. Men, all men, he thundered, are born to work.⁴⁶ Pedro de Calatayud seemed to enjoy taunting the rich, high born, and powerful. Most lived in mortal sin because of their habitual frauds and injustices and oppression of the poor and humble. Even worse were judges, magistrates, and politicians.⁴⁷ For these preachers wealth, rank, and power were a triple snare, so redolent of evil and injustice that those who possessed them were almost inevitably lost to God and virtue. Both Garcés and Diego José joined Pedro de Calatayud in his condemnation of luxury (*lujo*), which produced heresy and “cruelty against the poor and the needy.”⁴⁸

The preachers also warned notaries, lawyers, doctors, bureaucrats, and other professional men against injustice. Again Pedro de Calatayud was the most outspoken in condemning notaries who cheated clients, lawyers who accepted bribes or ignored conflicts of interest, and others who favored the rich, such as “those impious doctors who offer great care for the sickness of a wealthy patient or a person of importance but deal off-handedly with that of a poor artisan or country bumpkin.” In the *sentencias* he pronounced during processions Calatayud declared:

Be on guard, nobles and wealthy men, for it is already time to celebrate with tears the misery that awaits you. . . . The sweat of the servant or worker whom you have cheated of his pay clamors [for justice]; his cries have risen even to the ears of God!

Haughty and proud men! You have raised yourselves up and become conceited; you seek to dominate all and subject all to your lordship; you solicit nobilities and exemptions, you avoid charges and taxes in which you are meant to be included like all others; now you count yourselves mighty because you command or have a protector, but you will not endure; you will be confounded and humiliated; God will break your pride like a grain stalk that reaches above its neighbors.

Calatayud and, less frequently perhaps, the other preachers used such language in sermons not only to small groups of the rich and powerful but even more often to the large crowds in the cathedrals and open places, where, as Calatayud wrote, “the poor and miserable abounded.”⁴⁹ They damned the rich before their victims. While Calatayud concentrated on the injustices of the rich, he did not neglect the misdeeds of the poor. After repeatedly condemning the victimizers of poor peasants and shepherds (*pobres labradores, ganaderos, and pastores*), he lectured the poor who victimized each other or the wealthy. He condemned craftsmen—shoemakers, carpenters, weavers, bakers, and others—who committed frauds

⁴⁶ Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 167–201.

⁴⁷ Pedro de Calatayud, *Misiones y sermones*, 2: 293, 320, 390; and Diego José de Cádiz, “Idea de un caballero cristiano,” in *Colección de sermones y otras obras*, 5 (Madrid, 1796–99): 11, 38.

⁴⁸ Pedro de Calatayud, *Misiones y sermones*, 2: 297; and Diego José de Cádiz, “Idea de un caballero cristiano,” 46.

⁴⁹ Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 48–50; Pedro de Calatayud, *Catecismo práctico*, 258, and *Doctrina práctica que hizo el p. Pedro de Calatayud, misionero de la Compañía de Jesús* (Valladolid, 1748), p. 48 and chap. 11.

against customers and even "ordinary people or peasants" who sold faulty livestock.⁵⁰

The most vilified of all were merchants and other businessmen, including dealers in farm products. Calatayud, Garcés, and Diego José repeatedly attacked them for greed, sharp practices, and victimizing the poor, especially defenseless peasants. Again Calatayud was the most outspoken, as can be seen in his popular *Practical Catechism*, which four archbishops and a dozen bishops recommended to their clergy with the consequence that it was reprinted at least eight times during the century. In it he asked, "Who are the mice [*ratones*] of the countryside?" The answer was "those merchants and profiteers who, with their illicit gains, suck the blood of peasants, of noblemen who do not trade, and of townsmen who are not in business." Toward the end of his career, he wrote a long, learned essay, *Treatises and Practical Teachings on the Sale and Purchase of Merino Wool and Other Items* (1761), inspired "by having observed in several parts of Spain, on the occasion of conducting missions, many injustices and a great deal of illicit profiteering in the purchase and sale of wool and other goods." His essay was a guide to the confused who wanted to avoid committing injustices and a generalized exposé of frauds against the defenseless. Specifically and courageously, he singled out the merchants of particular communities in sermons and in his published *Doctrinas prácticas*. He attacked the merchants of Bilbao and other towns who cheated small ironmakers; the dealers in rye (the staple of the poor) who bought cheaply and sold dearly after scarcity drove up the prices; the speculators of Rioseco, Vitoria, and other towns who cornered the markets in sugar, cod, and other products; and all merchants who defrauded the public by unloading faulty or overpriced goods. He advocated a just price and naively suggested that merchants consult theologians to resolve their confusions about business practices.⁵¹

Miguel de Santander also wrote and preached about these issues. But he recognized, in conformity with medieval Christian doctrine, the legitimate goals of businessmen. His *Pláticas doctrinales* conceded that merchants could accrue licit profits from their own work. But, he warned, merchants must fulfill their obligations to the community, including the distribution of free grain to the poor in times of distress and, most important, avoidance of usury.⁵² Strictly interpreted, usury included many forms of profitable transactions besides the lending of money at interest. All of the preachers constantly stressed that usury was the businessman's greatest sin. In accordance with the urging of sixteenth- and seventeenth-century writers of preachers' manuals, Garcés and Pedro de Calatayud denounced usury during almost every mission. Calatayud wrote:

Various merchants and men of commerce suffer a similar illness, since the more they go on acquiring, the more they want. . . . The shame is they want to satisfy their appetites with

⁵⁰ Pedro de Calatayud, *Doctrinas prácticas que suele explicar en sus misiones el padre Pedro de Calatayud*, 3 vols. (Valencia, 1737–39), 2: tratado 15, p. 217, and pp. 218–20.

⁵¹ Pedro de Calatayud, *Catecismo práctico*, 255–56, and *Tratados y doctrinas prácticas sobre ventas y compras de lanas merinas y otros géneros*. . . . (Toledo, 1761), 1. Also see his "De las usuras" and "Del hurto," in *Doctrinas prácticas que suele explicar*, vol. 2, and his *Misiones y sermones*, 3: 301.

⁵² Miguel de Santander, *Pláticas doctrinales para misiones*, 272–73.

what does not belong to them. There is the same difference between a lion and a poor beast of burden as between a man of commerce who enriches himself through unjust dealings and the poor peasant in the countryside. . . . Just as the ass on the mountain is the prey of the lion with his claws, so poor peasants are the prey of nearby merchants who fatten themselves on their blood.⁵³

Both Calatayud and Miguel de Santander conceded that lending at moderate rates, especially in ports and commercial centers, could be legitimate. Even Calatayud admitted that such transactions might actually benefit the poor by encouraging commerce, industry, and employment. Yet both condemned usurious grain dealers and others who batted particularly on helpless peasants.⁵⁴ Having studied the issues in detail, both understood the complicated nature of commerce, appreciating better than Diego José and Garcés the intricacies of contracts that peasants and artisans concluded with the merchants whom they supplied—contracts that often led to accusations of usury. That theologians often disputed the nature of usury at this time is clear. Partly through papal intervention, the traditional concept of usury was undergoing significant change in the eighteenth century to accommodate the legitimate interests and sensitivities of businessmen,⁵⁵ changes that left conservative preachers, such as Calatayud, Garcés, and Diego José, increasingly out of step with current opinion. Their denunciations were unwelcome to the business community and to ministers in Madrid who encouraged commercial interests. The result was confusion and faux pas, particularly scandalous in the cases of Garcés and Diego José. Despite shifting attitudes, the preachers knew what they hated about the relationship between the powerful and rich trading community and peasants and artisans whom they regarded as essentially defenseless, though not always innocent of cheating. As Pedro de Calatayud implied, the rule of thumb must be that the poor and honest not be victimized by the powerful and unscrupulous.⁵⁶

The preachers frequently discussed two other important themes that reveal additional aspects of their social vision: giving charity and the reconciliation of disputes. Both offer ample evidence of the missionaries' involvement in the daily affairs of the people to whom they preached, and both provide deeper understanding of their mentality and popularity. The missionaries normally associated, personally as well as morally, with deprived peasants and artisans. Pedro de Calatayud, Diego José, and Garcés shared the plain life of the poor, with whom they often traveled and lodged during their missions. Although they sometimes stayed with the rich and aristocratic (Diego José once lodged, when ill, with his

⁵³ Pedro de Calatayud, *Doctrinas prácticas que suele explicar*, 2: 205–06; and Diego de Estella, *Modo de predicar*, 1: 123–25.

⁵⁴ Miguel de Santander, *Pláticas doctrinales para misiones*, 264–70; and Pedro de Calatayud, *Doctrinas prácticas que suele explicar*, 2: 207–08.

⁵⁵ "Representación hecha al rey n. sr. por los diputados directores de los Cinco Gremios Mayores," in Antonio Valladares de Sotomayor, ed., *Semanario erudito* (Madrid, 1787–91), vol. 27; J. T. Noonan, *The Scholastic Analysis of Usury* (Cambridge, Mass., 1957); and Vicente Palacio Atard, "Estilo de vida aristocrático y mentalidad burguesa," in his *Los españoles de la ilustración* (Madrid, 1964), 95–105.

⁵⁶ Pedro de Calatayud, *Doctrinas prácticas que suele explicar*, 2: 207–08.

patroness, the duchess of Medinaceli), they preferred to live with and to share the fare of ordinary fisherfolk, peasants, and artisans. Occasionally they feared becoming corrupted by the world of wealth and ease—for example, Garcés was kept preaching in Madrid far longer than he wanted and believed wise. In their home monasteries and while lodging elsewhere they lived frugally and enthusiastically so. By actually living in poverty and humility, they gained the support of the poor.⁵⁷ In sermons and conversation they stressed the material wants of the poor, the sick, the imprisoned, and the orphaned. They encouraged charity, received it, distributed it, and organized it on a more permanent footing. Their arrival in a town usually inspired the distribution of money, food, clothing, medicines, and other goods and a conspicuous, if temporary, show of concern by the rich for the humble. When Pedro de Calatayud preached in Betanzos, the count of Itre, viceroy of Galicia, helped him organize his mission and provided food for the poor who attended from out of town. At Braga, in Portugal, Calatayud helped organize food for prisoners, and in Madrid he distributed it personally to the jails, aided by leading prelates.⁵⁸ While preaching in New Castile, after finally taking leave of the court, Garcés deliberately sought out jails, asylums, and hospitals. Refusing to disdain even the humblest items, he not only organized the collection of medicines but also obtained and distributed tobacco to the insane. In Zaragoza, as was his custom, he sandwiched between the rosary and the sermon his appeal for charity, accounting for what had already been donated. Diego José was also effective in raising both goods and cash. Encouraged by his presence in Cádiz, the wife of General O'Reilly, military governor of the province, distributed cash daily to all comers before the church in Puerto de Santa María. During Lent in 1772, Diego José and his companion encouraged a large attendance at their mission with a generous dole of food. Diego José described events in a private letter from Ubrique:

I arranged to invite the poor on Sunday in order to give them some bread and fish because many were in need there. With this end in mind, I gathered sufficient grain and bread and, when all was ready, I preached the sermon, which was about charity, and, when this was over, I summoned them to the monastery and there came not only the poor but all the neighborhood and many from nearby towns. . . . Much of this bread and still more besides was distributed in many towns for the ill.

Being an effective organizer, Diego José was often consulted about what kind of charity should be given and to whom. Several prelates, the duke and duchess of Medinaceli, and other aristocrats and the wealthy sought his advice. Sometimes municipalities requested and received his suggestions. Consequently, Diego José

⁵⁷ Diego José de Cádiz, *Cartas íntimas del beato Diego José*, 69; Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 164–67; and Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 74.

⁵⁸ The duty of comforting and converting convicts was widely accepted by missionaries, both Catholic and Protestant. R. A. Mohl, "The Urban Missionary Movement in New York City, 1800–1825," *Journal of Religious History*, 7 (1972): 114.

deserves at least partial credit for the establishment of several institutions that aided the needy.⁵⁹

Finally, reconciliation was among the primary objectives of the Spanish preachers, as it was for seventeenth-century French missionaries. The preachers were proud of their successes in reconciling enemies, pacifying feuds, ending factionalism, bringing about the amicable settlement of lawsuits and legal claims, reuniting estranged couples and parents with children. They intervened to calm disputes among the clergy and between clerics and laity. Often, the poor sought out the preachers, begging them to intercede in litigation. The poor realized their last hope lay in the persuasive power of the preacher, when they were denied justice because they lacked money or influence or needed to collect debts from the powerful and "the judge or magistrate [paid] no attention," as Pedro de Calatayud wrote. And, like the missionaries studied by Ferté in the Paris region, the Spanish preachers considered the resolution of quarrels between priests and parishioners an important measure of a successful mission. To settle especially bitter or longstanding feuds approached the miraculous.⁶⁰

THREE OF THE FOUR MISSIONARIES found themselves at one time or another in serious conflict with the government. In sermons, some of which were later published, Pedro de Calatayud, Diego José, and Garcés accused businessmen of usury and profiteering; consequently, the preachers confronted both the monarchy and the mercantile community. Since all three incidents have been recently studied, only a brief outline will be presented here.⁶¹ In 1732 Pedro de Calatayud, after a mission in Bilbao, castigated local iron merchants for misconduct. Since they did not react, he published his admonitory sermon in *Doctrinas prácticas*, the relevant volume appearing in 1739. When he returned to preach in the region two decades later, the mercantile community, led by the Consulado, their protective association, complained about his sermons. Pedro de Calatayud attempted to defend himself against their charge of unfairness, and the issue was apparently dropped until 1766. By then, the Consulado had the support of some local clerics

⁵⁹ Torres Asensio, *Historia de la vida*, 22–23, 84, 101; Fernández-Anchuela y Collado, *Tres doctrinas prácticas*, 56–57, 63–64, 68; and Espallargas, *Vida del v. padre maestro fr. Antonio Garcés*, 126–27, 145–50.

⁶⁰ Pedro de Calatayud, *Misiones y sermones*, 1: 46; Ferté, *La vie religieuse*, 214–15; Torres Asensio, *Historia de la vida*, 20–21, 89–94; and Berthelot du Chesnay, *Les missions de Saint Jean Eudes*, 27, 168–72. Historians have shown how strongly communities pressed individuals not to resort to the courts to settle differences. Catholic and Protestant clerics sometimes played a major role in reconciliations without litigation. Alfred Soman, "Deviance and Criminal Justice in Western Europe, 1300–1800," *Criminal Justice History*, 1 (1980): 18–20; Bruce Lenman and Geoffrey Parker, "The State, the Community, and the Criminal Law in Early Modern Europe," in V. A. C. Gatrell et al., eds., *Crime and the Law: The Social History of Crime in Western Europe since 1500* (London, 1980), 21–22; and J. A. Sharpe, "Enforcing the Law in the Seventeenth-Century English Village," in Gatrell et al., *Crime and the Law*, 111–14.

⁶¹ William J. Callahan, "Utility, Material Progress, and Morality in Eighteenth-Century Spain," in David Williams and Paul Fritz, eds., *The Triumph of Culture: Eighteenth-Century Perspectives* (Toronto, 1972); Jean Sarrailh, *La España ilustrada de la segunda mitad del siglo XVIII* (Mexico City, 1957), 278–81; Juan Alvarez Junco, "La Sociedad Aragonesa de Amigos del País en el siglo XVIII," *Revista de Occidente*, 69 (1968): 301–19; Diego José de Cádiz, *Pláticas morales que el venerable y m. r. fr. Diego José de Cádiz . . . predicó en la ciudad de Zaragoza* (Madrid, 1817); and Diego José de Cádiz, *Cartas íntimas del beato Diego José*, 74–77.

and lawyers and appealed to the Council of Castile—Spain's supreme judicial body. Since Pedro de Calatayud was a Jesuit (whose order was under concerted attack at that time by enlightened reformers and others) and the council was heavily influenced by Campomanes, promercantile and anti-Jesuit minister, the issue was inevitably decided in favor of the merchants. In September 1766 a royal order forbade Pedro de Calatayud's return to the Basque province and banned his sermon from circulation there. Garcés committed his offense in Madrid and possibly in the presence of the king himself. Garcés, one of the oratorical wonders of his age, had been appointed in 1762 a court preacher to Charles III. But this honor did not affect Garcés's candor, and in a sermon delivered at court in 1763 he outraged the rich and powerful Five Great Guilds (*Cinco Gremios Mayores*) and presumably many who invested with them. The guilds dominated commerce and banking in Madrid. Garcés scandalized the town's financial elite by charging them with usury for collecting and paying interest—to the accused a quite normal practice. Appeals and counterappeals before the Council of Castile followed. The merchants—supported by some Jesuit, Franciscan, and other theologians—triumphed when a royal decree of 1764 vindicated the businessmen. Garcés left Madrid forever.⁶²

During a mission at Zaragoza in 1786 Diego José caused the most famous dispute of all by attacking the writings of the progressive economist Lorenzo Normante y Carcavilla, professor of political economy in the city's Economic Society. The conflict that followed had been foreshadowed in 1778, when local authorities reacted strongly to a sermon Diego José delivered in Granada criticizing aspects of royal reform policies. Campomanes, always vigilant against criticism, issued a stern warning to the Capuchin. Eight years later Diego José, apparently unwittingly, unleashed a far greater storm by denouncing from the pulpit certain works by Normante that defended luxury and usury and attacked clerical celibacy. Diego José had already disturbed the atmosphere by censuring the Enlightenment in a series of sermons, and the reformers counterattacked with the usual appeals to Madrid. Again the outcome was inevitable, and both Normante and the Economic Society were upheld to Diego José's embarrassment. Even more humiliating was the investigation that the Inquisition, at the instigation of the government, undertook into his teachings in 1801, shortly before his death. It was bitterly ironic that a principal accusation against him was that he had exaggerated the authority of the Inquisition itself.⁶³

⁶² By the second third of the century many Jesuit and other theologians favored a revision of medieval teaching on usury. Pitting himself against revision, Garcés was following his Dominican colleagues. Vicente de la Fuente, *Historia eclesiástica de España*, 6 (Madrid, 1873–75): 138–39; and "Representación hecha al Rey," in Valladares de Sotomayor, *Semanario erudito*, 244.

⁶³ Torres Asensio, *Historia de la vida*, 164–70, 301–05; Diego José de Cádiz, *Cartas espirituales del beato fr. Diego de Cádiz* (Madrid, 1945), 75–77; Diego José de Cádiz, *Cartas íntimas del beato Diego José*, 25, 30; Alvarez Junco, "La Sociedad Aragonesa de Amigos del País," 307–13; *Informe sobre la delación que hizo el p. Cádiz*, Real Academia de la Historia, Madrid, MS. 9–27–3–E95; and Archivo Histórico Nacional, Madrid, Inquisición, leg. 4449, exp. 1.

ALMOST FROM THE BEGINNING of the eighteenth century the Spanish clergy had been challenged by reformist, anticlerical, and later rationalist opinions and programs. During the last years of the War of the Spanish Succession, the reformer Melchor de Macanaz had criticized what he saw as clerical obstructionism. His successors, impelled by Spain's material needs and encouraged by successful reforms abroad, elaborated policies and wrote works that created an atmosphere hostile to many ecclesiastical interests. At first purely practical, the reformers increasingly imbued their arguments with the language and mentality of a doctrinaire antagonism to traditional Catholicism. This occurred in the second half of the century, during the epoch of Campomanes, Pablo de Olavide, Gaspar Melchor de Jovellanos, and other precursors of Spanish liberalism. Although at first many clerics found themselves allied with the reformers on a surprisingly wide range of issues, including university reform, reorganization of charity, and numerous economic programs, they rejected the philosophical and theological implications of enlightened reform and feared that some changes would make the church too vulnerable. Even clerics who were most disposed toward enlightened reforms, such as Miguel de Santander and his Jansenist colleagues in the body and hierarchy of the church, remained men of faith and spirituality, anxious about the implications of intellectual and material change. Anxiety and distrust became increasingly characteristic of clerical opinion during the later part of the century. Clerics saw themselves assailed and devalued on many sides; believing they must fight to protect their interests, they were increasingly driven to defend their church and confound their opponents. Powerful preachers were invaluable to the encouragement of traditional religiosity and loyalty to the clergy. They were impelled by the more traditional Spanish Catholic consciousness, a keen sense of evil, and an equally sharp sense of virtue. For them evil consisted mainly of sexual license and physical and moral corruption, greed and the immoderate pursuit of material goods, and the frauds and profiteering that materialism promoted; the source of all these evils was, in their view, disobedience against God's word, isolation from his church, and failure to repent. Virtue consisted largely of the opposites: belief in and obedience to God and his clergy; sexual purity and restraint of the passions; and, most pertinent of all to this study, the continued predominance of the agrarian, artisanal community and of its supposed social virtues. They believed such a community should be presided over by a Christian lay and clerical elite of talent, virtue, and spirituality. Most of its members—clergy with their mortmain, noble and gentleman landowners, and peasants and artisans in their villages and towns—would be, the preachers imagined, associated with the land and its products. They would form a single community of the godly, living harmoniously, directly or indirectly supported by the land on which they lived. Exploitation would cease. Businessmen had little role to play in this implicit scheme, and their presence would be a continual threat to Christian virtue. Merchants were necessary, to be sure, but most of them were sinners who victimized the innocent. Of the four preachers only Miguel de Santander had a moderate opinion of businessmen. H. D. Smith has pointed out that distrust of commercialism was common among popular preachers even before the eighteenth

century,⁶⁴ and those of Spain were not unique. It was the specificity and pertinence of the preachers' denunciations and the apparent impact they had on audiences that so agitated the business community and the latter's protectors, the government's reformist ministers.

The Christian and agrarian social vision of the Spanish preachers had strong roots in the Christian Middle Ages and the Aristotelian heritage. From the same source came the vision of Thomas More and the preference for rural society in English literature of the sixteenth and seventeenth centuries, which idealized the "'natural' or 'moral' economy . . . as a contrast to the thrusting ruthlessness of the new capitalism."⁶⁵ It was also closely akin to the agrarian social movement of Archbishop Fénelon and his colleagues in late seventeenth- and early eighteenth-century France. It was reminiscent of Protestant movements, such as the Moravian Brethren who fled the complications and temptations of commercial and industrial modernization to found peasant-artisan communities in Central Europe and the New World.⁶⁶ Finally, it was a variation of the native Spanish enthusiasm for the "bucolic ideal," "celebrating the virtues and loyalties of the rural scene and the country dweller." This enthusiasm received its fullest literary expression in the seventeenth century, maintained its vitality in the eighteenth, and found in Jovellanos, author and public servant, a powerful spokesman. Writing in the 1790s, he declared that an "immense rustic population" would provide a hard-working, rich, virtuous, and simple citizenry, immune to "that ferment of corruption that luxury always infuses in people."⁶⁷ But the preachers were, like More, Fénelon, and the Moravian Brethren, more than merely the protectors of a traditional vision of society. They were also the apprehensive offspring of a society hitherto apparently agrarian but now gradually being remolded by commercial and industrial growth. Moreover, the particular material allegiances of the preachers and other clergy were to the land and the peasant community from which they derived their wealth and income. Thus, by both intellectual tradition and material interest, the preachers could be expected to at least suspect and dislike the apparently threatening and politically well-protected commercial communities.

The preachers acted partly to protect the material interests of the entire church. What made them popular among peasants, artisans, and others who came to hear them was not this but rather the confirmation of their own righteousness that they found in the sermons and *sentencias*. Religion has often been a powerful weapon with which the deprived fight for the reestablishment of their "rights."⁶⁸ In this case the preachers consistently and insistently over several generations "suggested a vision of a different social order" than that which seemed to be developing in

⁶⁴ Smith, *Preaching in the Spanish Golden Age*, 121–24.

⁶⁵ Raymond Williams, *The Country and the City* (St. Albans, Eng., 1975), 49–64, esp. 57.

⁶⁶ Lionel Rothkrug, *Opposition to Louis XIV: The Political and Social Origins of the French Enlightenment* (Princeton, N.J., 1965), 242–50; and Donald F. Durnbaugh, ed., *The Brethren in Colonial America* (Elgin, Ill., 1967), 20–21, 33–38, 118–23.

⁶⁷ Michael Weisser, "The Agrarian Ideal in Eighteenth-Century Spain," in Harry C. Payne, ed., *Studies in Eighteenth-Century Culture*, 11 (1982): 383, 389–90.

⁶⁸ John M. Yinger, *Religion, Society, and the Individual* (New York, 1957), 67–68.

Spain.⁶⁹ Those who crowded together to hear them were, in effect, participants in massive demonstrations of protest—of the only kind, needless to say, permitted by Bourbon absolutists—against what they perceived to be exploitation by commercial interests. Peasants were repeatedly reminded of their virtue—or at least potential virtue—by the missionaries, whose sermons depicted the rural poor as relatively untainted by economic sin. The preachers defended them against injustice not only in words but also by deeds of charity and by intervention with judges and magistrates. It is not surprising that these preachers could stir so deeply their mainly peasant and artisan audiences.

As teachers and propagandists of social virtue, the preachers defended traditional Christian morality and protected the interests of both owners and workers in agrarian society. But they might be better understood not only in their Christian historical, material, and literary contexts but also in that of contemporary secular political thought. J. G. A. Pocock has argued persuasively that secular Aristotelian conceptions of social virtue survived with vitality, particularly in the English-speaking world, at least through the eighteenth century. “Old” British and American Whigs and their successors—including the Scots John Millar, Adam Ferguson, and Adam Smith—witnessed in that century a growth of material culture, education, and economic sophistication and specialization whose negative effect on virtue they feared. They believed secular virtue was increasingly threatened by a chaos of appetites and formulated what Pocock aptly called “the country ideology,” one “that presented virtue as ever threatened by corruption.”⁷⁰ Whence emanated this corruption? Largely from the sophistications of commercial society and bureaucratic government—banks and easy credit (that is, widespread usury), trade, and rampant materialism, among other manifestations. “Their language was humanist, their enemy was modernity, and their posture has something of the sixteenth century about it and something of the twentieth.” If the missionaries’ vocabulary had not been Christian, Pocock might have been describing the Spanish preachers, at least all but Miguel de Santander. The latter, just like some of the thinkers Pocock analyzed, sought and found a moderate compromise between Christian secular virtue and commercial wealth. Unwilling to condemn prosperity as their predecessors had, the moderates eventually constructed a “complex formula . . . required in order to bring virtue and commerce together.” But among the Spanish preachers, as among the thinkers of Britain and North America, some never made the compromise. The conception of Christian social virtue held by Pedro de Calatayud, Garcés, and Diego José inevitably clashed with the demands of commerce. Such men suffered the pain of what Pocock called the “dread of modernity.”⁷¹

⁶⁹ Bryan R. Wilson, *Magic and the Millennium: A Sociological Study of Religious Movements of Protest among Tribal and Third World Peoples* (London, 1973), p. 6 and chaps. 1–2; and Neil J. Smelser, *Theory of Collective Behavior* (New York, 1963), 325–34.

⁷⁰ Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, N.J., 1975), 467, and “Virtue and Commerce in the Eighteenth Century,” *Journal of Interdisciplinary History*, 3 (1972): 130. Also see *Machiavellian Moment*, chaps. 14–15.

⁷¹ Pocock, *Machiavellian Moment*, 470, 477, 509.

Clearly, fear of businessmen and of their social impact was a widespread phenomenon of the age, confined neither to Catholic cultures nor to backward economies. Defense of Christian and Aristotelian social virtue was hardly the self-indulgence of religious fanatics. It was a realistic attempt by intellectuals and propagandists to defend a familiar agrarian society from economic change, which only hindsight now tells us was inevitable. Their "dread of modernity" and defense of agrarian society made the preachers into conservatives. Even before reaction against the French Revolution manifested itself in the 1790s, clerics, like Fray Fernando de Zevallos, had self-consciously championed traditional relationships and authorities against the threat of disorder and freedom. During and after the revolution, clerics took a leading role in formulating and teaching antirevolutionary doctrines. Pedro de Calatayud's earlier, unambiguous denunciation of the proud and powerful became heavily muted by the 1790s, as clerics spoke and wrote in defense of their king and church. Among the most influential conservatives were Simón Pérez, a priest of Murcia; Pedro Quevedo y Quintano, bishop of Orense; and Lorenzo de Hervás y Panduro, a scholarly Jesuit in exile from 1767. In the 1790s Diego José strongly deplored the inclination to abolish hierarchy, order, law, and obedience to the king. Indeed, Diego José has been seen as an important man of action, one of those who mark the transition from criticism of enlightened thought to establishment of a new mode of conservative action. His ascetic passion made him an ideal preacher now pitted against political as well as social and economic sin.⁷² For him, the French Revolution and incipient liberalism were the children of enlightenment and libertinage. The latter were the offspring of greed, physical and moral corruption, and the desire to escape from God. Therefore, revolution and ungodliness were inextricably related.

Later, after all these preachers save Miguel de Santander were dead, clerical reaction and the popularity of many clerics among ordinary peasants and artisans produced their final fruits. After 1808 came the uprisings of Spaniards who, pushed by clerical and lay firebrands, began the struggle against Napoleonic intervention in Spain. These clerics boasted of their patriotism and of the powerful effect their sermons and other exhortations had on the masses. A canon of Málaga cathedral proclaimed in 1809 that he had been one of the first "to hoist the colors of religion and justice, going out into the streets and squares to preach" and persuading more than five hundred men to enlist as soldiers against the armies of French atheists, which aimed "to introduce the spirit of libertinage by which they themselves are moved."⁷³ Vindicated by their popularity, rejecting modernity as in the previous century, protecting the interests of agrarian society and of the church, the clergy of the so-called War of Independence had indeed transformed the preachers' conception of social virtue into a clarion call of antirevolutionary action. Thereby they provided an invaluable defense of Bourbon authority, hierarchical society, and ecclesiastical power.

⁷² On Diego José, see Javier Herrero, *Los orígenes del pensamiento reaccionario español* (Madrid, 1973), 142-47. On other clerics, see *ibid.*, 91-104, 251-56, 274-79.

⁷³ Martínez Albiach, *Religiosidad hispana*, 19-34, esp. 30.

But the preachers of the decades from the 1730s to the 1780s were not yet the defenders of Bourbon absolutism, and the posture of Pedro de Calatayud, Garcés, and Diego José posed a challenge to governmental authority in those years when Bourbon ministers unambiguously defended many enlightened reforms. Under both Charles III and Charles IV the inclination of the monarchy was to suspect missionaries, to sustain businessmen in their complaints against them, and to punish and humiliate them as wrongdoers, or, as in the case of Pedro de Calatayud, to attempt to moderate their influence. The well-known policy of successive mid- and late eighteenth-century Spanish ministries was to protect and foster trade and industry and to sustain the interests of business communities (though not necessarily at the expense of agriculture, particularly when the latter was progressive and improving). Such a policy inevitably led to clashes with those who distrusted and condemned businessmen. After 1766 the monarchy had yet another reason for its position. The riots of that year badly frightened the government, for they demonstrated the extent to which popular clerics and discontented masses could effectively cooperate to bring about disorder and threaten stability.⁷⁴ Preachers who, like those studied here, possessed the power to persuade and excite crowds, often large ones, were always potentially dangerous. How easily discontent and resentment could manifest themselves when the preachers appeared is shown by an incident described by Diego José himself. At Málaga in 1778 his mission aroused little interest until he delivered a sermon that set the entire city in motion:

That night the religious communities, not only of nuns but also of monks and friars, offered up prayers and performed spiritual exercises, their eyes full of tears; some [laymen] went to the *calvario*, others to their homes, all of them agitated. . . . People made innumerable vows, offered rogatives, made general confession.

The following day, a Monday, the day of the penitential procession, appeared like a Holy Thursday; the churches—full of people; the stores and workshops closed. . . . The procession was formed [but] was delayed in setting off for nearly an hour and a half because of the great multitude of those who gathered; we arrived at the square, the site set aside for the sermon, where all Málaga was waiting. . . . I went up onto the balcony of the town hall, and while I was preaching it happened that the captain general [Alejandro O'Reilly, notorious among the masses for his tough action against the rioters of 1766 in Madrid] . . . arrived in his coach preceded by two soldiers as lifeguards with drawn swords. The entire crowd, which is believed to have numbered more than 20,000 souls, rose up; they began

⁷⁴ The riots occurred at Easter, 1766, and were "a major crisis for royal authority." The Madrid incident was known as the uprising of Esquilache, after the hated, tough, reformist minister of finance whom the crowds forced out of power and into exile. Barcelona, Cádiz, Zaragoza, and scores of other cities and towns witnessed days of rioting sparked by high food prices, high taxes, unpopular financial and other reforms, and, in Madrid, the manipulation of popular feelings by self-interested aristocratic and clerical groups. In some places, such as Cuenca, clerics and the rebels openly cooperated, and in Madrid a leading spokesman for the crowd was an ecclesiastic. In many cases the major targets of the rioters were local officials, grain merchants, and money lenders who had kept grain and other food prices high. Charles III fled from Madrid, and it was feared his throne might be in danger. Subsequently, the king and his ministers made the Jesuits scapegoats and for this and other reasons expelled them from the monarchy in 1767. See Laura Rodríguez, "The Spanish Riots of 1766," *Past and Present*, 59 (1973): esp. 124–27, 129–30. For an older but more detailed account, see Antonio Ferrer del Río, *Historia del reinado de Carlos III en España*, 2 (Madrid, 1856), bk. 2: chaps. 1, 2, 4.

to riot, and it was very close to a rising in which there would have been a thousand fatalities since armed troops were also standing by.

I was greatly shocked, but the Lord gave me the strength to contain the crowd, which, calmed by my speaking, allowed [the captain general] to pass unmolested.⁷⁵

Such outbreaks were infrequent, but they served nevertheless to remind the government of the dangers lurking behind the popular preachers and the crowds they drew. It was no wonder that ministers distrusted the preachers who, with their “dread of modernity” and capacity to attract large crowds, posed a challenge to the economic aims and political stability of the ruling order.

⁷⁵ The previous day the captain-general's troops, attempting to control the crowds, had caused a number of injuries; Diego José, as quoted in Torres Asensio, *Historia de la vida*, 59–60.

Review Article
The Personality of Atatürk

KEMAL H. KARPAT

VAMIK D. VOLKAN and NORMAN ITZKOWITZ. *The Immortal Atatürk—A Psychobiography*. Chicago: University of Chicago Press, 1984. Pp. 359. \$37.50.

THE EXTRAORDINARY PERSONALITY AND ACHIEVEMENTS of Mustafa Kemal Atatürk (1881–1938) have been the subject of numerous studies. At the peak of his power in the 1930s Atatürk was considered one of the great men of his epoch, although he would not have liked to be placed in the company of Mussolini, whom he held in contempt, or Hitler, whom he regarded as mentally deranged.¹ Early writings about Atatürk, both in Turkey and abroad, deal with his victorious campaign against the occupation forces of Greece, France, and England and the reforms. Other works examine his struggle with the sultanate, especially after the sultan became a friend of the Allies in June 1919 and opposed the nationalist resistance movement, which was directed from Ankara by an elected national assembly headed by Atatürk. Much of the historical information in these early writings comes from Mustafa Kemal's own six-day *Speech* (1927), which, though a basic source, offers the official view of the Turkish War of Liberation, 1919–22, the establishment of the republic, and events thereafter.²

After Atatürk's death, memoirs by his associates (and at times rivals) and friends—such as Ali Fuat Cebesoy, Kâzım Karabekir, the woman writer Halide Edib Adivar, the journalist Falih Rifki Atay, Atatürk's woman companion and secretary, the late Afet Inan (Afetinan), to mention just a few—brought to light new information about both the War of Liberation and Mustafa Kemal's personality.³ These memoirs were followed in the 1960s by two monumental

¹ Atatürk offered asylum to many German Jewish intellectuals and quickly stopped a group that engaged in anti-Semitic propaganda in a Turkish town.

² See Atatürk, *A Speech Delivered by Ghazi Mustapha Kemal, President of the Turkish Republic, October 1927* (Leipzig, 1929).

³ See Cebesoy, *Sınıf arkadaşım Atatürk: okul ve genç subaylık hâtıraları* [My Classmate Atatürk: Recollections from School and Early Days as Army Officers] (Istanbul, 1967); Karabekir, *İstiklâl Harbimizin esasları* [The Foundations of Our Independence War] (Istanbul, 1933–51); Adivar, *Memoirs*

works: Lord Kinross's *Atatürk, A Biography of Mustafa Kemal: The Birth of A Nation* and Şevket Süreyyâ Aydemir's three-volume *Tek adam, Mustafa Kemal*.⁴ These studies also provided new perceptions of Atatürk and the early history of republican Turkey. There are, of course, thousands of other books and articles (a recent bibliography lists 7,010 titles) that contain, on occasion, valuable information and insights into Atatürk's complex personality, including what he thought—apart from his belief in the importance of a secular state—about religion and, specifically, Islam. Several excellent works on Atatürk, to be mentioned later, appeared in the 1970s.

Vamik D. Volkan and Norman Itzkowitz's *The Immortal Atatürk* approaches the study of Atatürk from a novel angle. Volkan, a psychoanalyst at the University of Virginia, and Itzkowitz, a historian at Princeton with training in psychiatry, have attempted to analyze Atatürk's life and public career from a Freudian perspective. Both authors claim to have originally had an idealized picture of Atatürk. To them, he "represented a repository of some idealized aspects of ourselves" so that to "study him would be first of all a study of ourselves" (p. xix). Obviously the authors' approach demanded, first, a careful and critical study of Atatürk's relations with his mother and father and, second, the establishment of a linkage between his childhood and his career as an officer in the Ottoman army, a leader in the War of Liberation, and finally a reformist statesman. Since the authors contend that childhood and family relations conditioned and guided Atatürk's later actions, it is appropriate to discuss his formative years.

Atatürk was born in Salonika, which became part of Greece in 1913. The exact month and day of his birth is unknown, although later he decided it was May 19—a date coinciding with his landing at Samsun and the beginning of the War of Liberation, according to the first sentence of the famous *Speech*. Atatürk's father, Ali Rıza, was a customs official, then a rather unsuccessful timber businessman. Both professions required the courage to live and travel in Balkan mountains infested by bandits; hence, Atatürk saw his father as a heroic character. Late in life Ali Rıza married a teenager named Zübeyde, who gave birth to several children, of whom only Mustafa Kemal and his sister Makbule survived. His father died when Mustafa Kemal was only seven years old, but not before Ali Rıza decided to send his son to a "modern" school rather than to a religious classroom ruled by a *hoja* (clergyman), as his devout but little-schooled mother had wished. The modern school was headed by one Şemsi—the "Illuminator"—a name derived from the Arabic word *shems* (sun). Thus, Atatürk referred to the sun and its life-giving power in many of his speeches. At school the name Kemal (the "Perfect") was added to his initial name of Mustafa (the "Chosen") in order to

of Halidé Edib (New York, 1926); Atay, *Çankaya: Atatürk 'ün doğumundan ölümüne kadar* [Çankaya: From the Birth of Atatürk to His Death] (Istanbul, 1969); and Afetinan, *The Emancipation of the Turkish Woman* (Paris, 1962).

⁴ Lord Kinross, *Atatürk, A Biography of Mustafa Kemal: The Birth of a Nation* (New York, 1965); and Aydemir, *Tek adam, Mustafa Kemal* [Unique Man, Mustafa Kemal] (Istanbul, 1963–65). A reprint of Kinross's *Atatürk*, which appeared in the same year, is entitled *Atatürk: A Biography of Mustafa Kemal, Father of Modern Turkey* (New York, 1965).

distinguish him from a teacher or colleague who bore the same name. The untimely death of Mustafa's father left the family in economic need, and, consequently, they moved in with an uncle who was the caretaker of an absentee landlord's estate. Eventually Mustafa Kemal returned to Salonika and enrolled in another modern school, where his behavior reflected his "idea that he had to be self-sufficient, special, different and above other children" (p. 34). Later, again over his mother's opposition, he chose a military career, impelled by "his inflated self-concept" (p. 35) and following the wish of his idealized father, who symbolically dedicated "Mustafa to the military by hanging his sword alongside his crib" (p. 35). Atatürk's mother remarried, which disturbed him, and relations between them remained cool until the stepfather died and Mustafa's love and devotion to his mother resurfaced.

By the time Mustafa Kemal graduated from military school his personality had emerged. According to Volkan and Itzkowitz, his father represented the ideal—the good, the brave, and the progressive. His mother, on the other hand, though very attached to her son and enjoying until the end of her life Atatürk's love, devotion, and respect, was the prey of the forces of oppression, obscurantism, and backwardness—as was Turkey, eventually occupied and threatened with extinction by foreign invaders. Mother and mother-country became synonymous in Mustafa Kemal's unconscious mind as he strove openly for the salvation and emancipation of the latter. "Atatürk's undying, unconscious selfish childhood wish was to rescue his mother, to be her savior. It was a selfish wish because underneath it represented his desire to have a repaired mother who would be able to be a more nurturing mother to him" (p. 126). Zübeyde died in January 1923, after the country had been liberated from foreign occupation, the sultanate abolished (1922), and the other oedipal father, the sultan, forced to flee the country. The future appeared promising for Turkey and for Atatürk. He had achieved his own and the country's liberation, because the "image of the mother who die[d] continue[d] in the image of the nation which survive[d]" (p. 217).

In essence, the authors describe Mustafa Kemal as narcissistic, with an inflated self-concept. Fortunately, these traits proved useful in the struggle to establish the Turkish republic. Atatürk sought to identify himself with the nation by giving it his own vision of freedom and progress—freedom from the shackles of Islam and progress brought by the adoption of Western civilization. Married but a short while and childless, Mustafa Kemal assumed the name Atatürk—Father of the Turks—bestowed on him by the National Assembly in 1934. Thus, his full name became, literally translated, "the Chosen, the Perfect, the Father of the Turks." (Surprisingly, the authors failed to take note of this.) Mustafa Kemal also had the title *Ghazi* (the "Victorious") given him by the assembly in 1921 after his defeat of the Greek army.

It is impossible to summarize here all of the arguments and evidence used by the authors to interweave the psychological origins of Atatürk's actions and thoughts with the history of the nation as a whole. In search of new material, Volkan conducted a series of interviews with Atatürk's adopted daughters, friends, and associates, as well as orderlies, maids, and other individuals who knew or had

personal contact with him. These interviews provide new and excellent information about Atatürk's personal life, including his relations with women. (Considering who he was and the popularity he probably had with women, whose position improved under the republic, his relationships with them appear to have been unsensational.) But Volkan and Itzkowitz interpret the information in a way that sustains their thesis, at times without regard for obvious incongruities between historical events and Atatürk's psychological features.

Actually, much of Atatürk's behavior, as a child and as an adult, and the genesis of his ambitions can be explained equally well—if not better—by relying on a cultural-sociological approach rather than on Freudian analysis. For instance, his family background, childhood experiences, and the attitudes he held toward his mother and father (and they toward him) appear to have been in basic harmony with Turkish culture. In the Turkish society of those days the marriage of young women to older men was not uncommon. There was a high rate of mortality among infants and children, but many of those who survived subsequently became fatherless, for the mortality rate among men, mainly ethnic Turks who fought the incessant Ottoman wars of those years, was also high. Town and village women were then—and still are—more religious and more steeped in the traditional customs than men, and, then as now, mothers tended to be particularly attached to their sons, who, the women believed, would assume the duties of protecting and providing for the family on the death of the husband-father (as well as perpetuating the family line). Fathers were traditionally the authority figures who commanded obedience and respect. They were also the agents who dealt with the outside world and the models for their sons' behavior in society. Still, women, despite their apparent servility, often had the decisive voice in familial affairs. Schools were, and still are, less influential than the family in the transmission of behavioral norms. Atatürk's interest in an army career was, again, not unusual considering the atmosphere of the time. The Ottoman leadership's efforts to create a territorial state and bonds of political and cultural loyalty among the citizenry—and, thus, a national Turkish identity—were well underway by the time Atatürk was born. Namik Kemal's famous play, *Vatan* (Motherland), to which the authors allude in a different context, illustrates the effort to foster attachment to the state by substituting nationality for religion. Army officers were regarded as the symbols of progress, the potential saviors of the motherland; consequently, they enjoyed great prestige and provided role models for young men.⁵ In sum, the development of Mustafa Kemal's personality and his ideas of reform appear to have resulted more from the existing sociopolitical and historical conditions than from oedipal complexes. In fact, many of the reforms that he initiated had been proposed and discussed earlier by a variety of intellectuals, including suggestions for adopting the Latin alphabet, which Atatürk did in 1928 as a means of quickly increasing literacy among Turks. The Latin alphabet fitted Turkish phonetics and could be learned faster than the Arabic. That the abolishing of the Arabic alphabet

⁵ For an excellent account of the army's prestige and the interest of Turkish youth in an army career, see Şevket Süreyya, *Suyu arayan adam* [The Man Seeking the Water] (Istanbul, 1967).

corresponded to Atatürk's own psychological urge to liberate his mother and the country from the vestiges of obscurantism and superstition is coincidence.

Most of the book, or roughly twenty-five chapters out of a total of thirty-one, concerns historical events; consequently, it is necessary to deal with these at some length. The authors obtained their basic historical data from a variety of Turkish and foreign sources and original interviews. These sources, like most of the books on Atatürk published before the 1970s, place Mustafa Kemal at the center of events, which is also the tone of the *Speech*. In the 1970s, however, radical, Marxist, and extreme religious groups in Turkey, bent on undermining the regime in favor of some other type of rule, attempted to discredit Atatürk and his achievements, since Atatürk and his multifaceted legacy provide the chief ideological and political linchpins of the Turkish national state and society. The radicals claimed that the War of Liberation was not a creation of Atatürk but a national populist movement headed by a coalition of army officers, religious leaders, and intellectuals. This view was shared by many moderates and liberals, although these groups were firmly committed to the maintenance of the Atatürk legacy. At the same time, the position of the anti-Atatürk forces was strengthened by the policies of the Republican People's party (RPP), which, after touting Atatürk's ideals (especially secularism) for twenty years in its campaigns for votes, shifted its orientation first to a position left of center, then toward social democracy, and, finally, toward socialism. Atatürk founded the RPP and used it to gain political control and achieve reform based on the ideas of nationalism and secularism. The Atatürk legacy remained the party doctrine until 1971–72 when İsmet İnönü and his group lost in party elections to Bülent Ecevit and his leftist supporters. Thus, when the RPP swung to the left, the vehicle for organized political support of Atatürk's policies was destroyed, although the country as a whole remained generally faithful to Atatürk and the republic and the military maintained its complete dedication to his ideals.

Under these circumstances, many intellectuals loyal to the Atatürk legacy felt required to develop a new approach to the study of the republic's history that would show Atatürk's relationship to other social groups and Turkey as a whole. The resulting books and articles succeeded in depicting Atatürk and the War of Liberation in a broader political, social, and cultural context and highlighted in a convincing manner his foresightedness and leadership qualities. Among these works, Mahmud Goloğlu's multi-volume history (based on a great number of documents), Doğan Avcioğlu's interpretive work, Sabahaddin Selek's *Anadolu İhtilali*, and Muzaffer Gökman's bibliographical compendium deserve special attention.⁶ These new books point out that organizations in the countryside for

⁶ Goloğlu, *Milli Mücadele Tarihi* [The History of National Struggle], volume 1: *Erzurum Kongresi* [The Congress of Erzurum] (Ankara, 1968); volume 2: *Sivas Kongresi* [The Congress of Sivas] (Ankara, 1969); volume 3: *Üçüncü Meşrutîyet, 1920* [The Third Constitutionalism, 1920] (Ankara, 1970); volume 4: *Cumhuriyet Doğru, 1921–1923* [Toward the Republic, 1921–23] (Ankara, 1971); volume 5: *Türkiye Cumhuriyet Tarihi, 1923* [The History of the Turkish Republic, 1923] (Ankara, 1971); Avcioğlu, *Milli Kurtuluş Tarihi (1838'den 1995'e)* [The History of National Liberation from 1838 to 1995], 3 vols. (İstanbul, 1974); Selek, *Anadolu İhtilali* [The Anatolian Revolution] (4th edn., İstanbul, 1968); and Gökman, *Atatürk ve Devrimleri: Tarihi Bibliyografyası* [Atatürk and the Reforms: The Historical Bibliography] (2d edn., İstanbul, 1968). Also see Goloğlu, *Devrimler ve Tepkileri (1924–1930)* [The

resistance to occupation began to form before Atatürk's landing at Samsun; that the crucial congress in the summer of 1919 in Erzurum, which elected Mustafa Kemal as chairman and put forth the National Pact, was organized not, as Volkan and Itzkowitz claim, by Kâzım Karabekir, the commander of the eastern army, but by the Defense of Rights Associations of Trabzon and Erzurum; that some members of the congress put forth a proposal for social and economic reforms and decentralization, which was not discussed at the time but has nurtured the opposition to Atatürk's policies until our day.

It should be emphasized that liberation provided the foundation for civilian participation in government and the establishment of a new civic society. Atatürk resigned his army commission and conducted the war as a civilian under the formal supervision of the civilian National Assembly. When he took over the army command at the crucial battles of Sakariya and Dumlupınar, he did so at the direction of the National Assembly—hence, the Turkish military's penchant for subordination to elected civilian government. Volkan and Itzkowitz's casual dismissal of the role of Islam and of religious leaders in the War of Liberation and their emphasis on secularism as the key ideological force of Turkish politics is misplaced. Many religious leaders were originally close allies of Mustafa Kemal, both as delegates to the crucial congresses of Erzurum and Sivas and as chairmen and members of the Associations for the Defense of Rights that provided the civilian organizational foundation for the War of Liberation and later for the People's party. (The influence of the religious leaders, however, all but vanished by 1925.) Atatürk was not against Islam but against those who, on the basis of religious arguments (which, in fact, were the only arguments that could legitimately be used to urge the reestablishment of the sultanate and caliphate), challenged his leadership or questioned his growing personal authority. One thing is certain about Atatürk: he did not tolerate any political opposition, however legitimate it might be, despite the short-lived experiment with the Free or Liberal party established in 1930 at his urging by his close friend Fethi Okyar. These new studies, and I have cited only a few, are indispensable to any study of Atatürk and the early days of the republic. Unfortunately, Volkan and Itzkowitz not only failed to use these works but neglected even to hint at the new trends of thought in Turkey about Atatürk, the War of Liberation, and Islam. Instead, they adhered rigidly to the stereotypical views evident in the early studies of the formation of the Turkish republic.

A word should be said about Volkan and Itzkowitz's presumption that Atatürk was not interested in economics. It should suffice to mention that he initiated the Economic Congress, held in Izmir in 1923, which sanctioned his views, expressed shortly before the meeting, on both the social classes and the economic regime. Moreover, the failure of İsmet İnönü, the premier, to accomplish economic development (because of excessive centralization and heavy bureaucratic controls)

Reforms and the Reactions, 1924–30] (Ankara, 1972). Although Goloğlu has published other works covering the period after 1923, these are of less value.

led in 1937 to his dismissal and replacement by Celal Bayar, chairman of the Demokrat party and president of Turkey from 1950 to 1960. Bayar was the head of the semiprivate but successful enterprise known as the İş Bank (Labor Bank). Atatürk was quick enough to recognize the need for economic expertise.

MY CRITICISMS SHOULD NOT BE CONSTRUED as a condemnation or rejection of this book. On the contrary, Volkan and Itzkowitz, whose sympathy and admiration for Atatürk are obvious, have given his life and achievements a new human dimension. To their credit, they have not followed the general trend of depicting him in mythic proportions. After reading the book one has a better understanding of his character and his overwhelming dedication to the progress and welfare of his nation. Although I hold to my opinion that Atatürk's genetic inheritance, the social environment in which he developed, and the demands of his adult life, rather than psychological pressures, shaped his personality and public behavior, I admit wholeheartedly that the authors have provided excellent insights into his personality. To set the tone of their work, they selected as an epigraph a paragraph from one of Atatürk's speeches that I also find extremely revealing:

Man as an individual is condemned to death. To work, not for oneself but for those who will come after, is the first condition of happiness. . . . Each person has his own preferences. Some people like gardening and growing flowers. Others prefer to train men. Does the man who grows flowers expect anything from them? He who trains men ought to work like the man who grows flowers.

In sum, despite its shortcomings, this is a good and important book. It summarizes aptly many issues related to Turkish political and social life, and it is exceptionally well written. It is, in fact, one of those rare books that might be read with profit by both sophisticated scholars and the general public. Together with Lord Kinross's book, which it does not supersede in any way, it is one of the most important sources in English on modern Turkish history and on Atatürk, as well as the first to apply psychological analysis to Turkish history.

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Reviews of Books

GENERAL

KRZYSZTOF POMIAN. *L'ordre du temps*. (Bibliothèque des Histoires.) Paris: Gallimard. 1984. Pp. xiv, 365. 120 fr.

The fundamental presupposition of Krzysztof Pomian's book is that our knowledge of time is not limited to a single viewpoint but that there is a plurality of perspectives from which to view time. Not only is there the understanding of time from the perspective of a physicist with an atomic clock, but there is also psychological time, sociological time, philosophical time, and theological time, as well as the time of the historian who thinks in terms of events, eras, or even civilizations. Thus, as Pomian succinctly states: "This book is not about the idea of time. It is about time itself" (p. xii). As such, Pomian's work is not for those historians concerned with gaining an encyclopedic knowledge of the past but for those with a philosophical predilection.

Indeed, the book is primarily a neo-Kantian phenomenological study of the nature of time and its various interpretations by noted thinkers from Augustine to Martin Heidegger, from Ibn Khaldūn to Giambattista Vico and Albert Einstein. Pomian considers these various views of time under four main categories: (1) chronometry, which is concerned with various instruments of measuring time, from sundials to atomic clocks; (2) chronography, which deals with chronicles and varied accounts of events; (3) chronology, which is concerned with dates, names, events, and periods of history and embraces the totality of the past; and (4) chronosophy, which is concerned with global time and the history of man in the universe from the perspective of, for example, a cyclical or linear view of history.

Pomian also notes that some calendars are based on the sun, some on the moon, and others on a combination of the sun and moon. For some cultures the first day of the new year is an arbitrarily chosen date, and for others it is based on the summer or winter solstice. Chronometric time is therefore cyclical time, even if we view events in a

linear fashion. Indeed, linear time arises, Pomian believes, because of an inability to write about affairs over a long period of time. Thus, some years in calendars are designated by the name of consuls (as in Rome) or by the archons (as in Greece) or by great events (such as the Christ-event in Christianity and the revolution in French or Russian history). These obviously arbitrary ways of measuring calendar time, as Pomian rightly notes, will doubtless affect the way historians accomplish their tasks.

This work will be of especial benefit to those interested in the problems of historiology and historiosophy and ought to be read by those interested in historiography. Pomian offers a penetrating and informative analysis of the importance of human conceptions of time for the discipline of history.

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AUDREY B. DAVIS. *Medicine and Its Technology: An Introduction to the History of Medical Instrumentation*. (Contributions in Medical History, number 7.) Westport, Conn.: Greenwood. 1981. Pp. xiv, 285. \$37.50.

During the past 150 years the medical doctor has been transformed from sagem to technologist. Symbolic of this change are certain technological artifacts now admired and studied in museums and regarded as desirable by collectors.

Audrey B. Davis has organized her book on the technology of medicine in three parts: historiography, significant instrumentation, and the impact of medical technology. Her central theme is the development of the most important diagnostic instruments used by the physician in the nineteenth and twentieth centuries: the thermometer, the stethoscope, and pulse-measuring devices. By way of extended introduction she begins the book with an account of the literature of medical instrumentation and a highly selective description of the history and content of some medical museums. The final third of the book is more theoretical, dealing with the role

of instruments in diagnosis and in establishing medical standards.

This is a large-format book with some one hundred illustrations. It is irresistible to point out that figure 10.2 showing an obstetric kit strangely includes a sugar-loaf cutter. There is no separate bibliography, but the notes, gathered together by chapter, provide an ample reading list for the diligent student. There is a thorough index.

For the historian the value of this book lies in its enunciation of the fundamental changes in medical practice that occurred during the nineteenth century. These depended on the use of instruments and medical technology and were "1) diagnosis and treatment of many diseases with the aid of instruments, 2) the definition of disease based on the data supplied by instruments, and 3) the demarcation between health and disease according to the limits determined with instruments applied in the course of physical examination given both to healthy and sick individuals" (p. 235). Davis rightly points out that medicine reflects the economic and social circumstances of the periods and places in which it is practiced. The influence of the Industrial Revolution and the growing importance of public health and health-related legislation transformed medical practice and practitioners. The manual skills hitherto disparaged by physicians gradually grew in importance as diagnosis involving measurement became crucial to medicine, a change that was to "democratize" the profession.

The sections on medical museums are rather weak. If such an account be attempted, it should give an accurate record of the large number of historical medical museums that exist. The *Directory of Museums* by Kenneth Hudson and Ann Nicholls (1975) includes eight columns under the heading "Medical Sciences," three for Europe and one and one-half for the United States. Clearly, not all the museums listed will be relevant to the type of instrumentation dealt with here, but to refer to only two museums in Continental Europe is to present a distorted picture.

Since science progresses with the aid of instruments, a study of the devices used in medical science is overdue. This book is most welcome as an important contribution to the now rapidly expanding field of the history of scientific instrumentation.

GERARD L'E. TURNER
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GEORGE GAYLORD SIMPSON. *Discoverers of the Lost World: An Account of Some of Those Who Brought back to Life South American Mammals Long Buried in the Abyss of Time*. New Haven: Yale University Press. 1984. Pp. viii, 222. \$25.00.

George Gaylord Simpson was an outstanding paleontologist and one of the foremost writers on Darwinian studies and the history of his discipline. In 1980 he published *Splendid Isolation: The Curious History of the South American Mammals*; shortly before his death in 1984 he wrote *Discoverers of the Lost World*, which is more a companion than a sequel to his previous work.

This new book is mainly concerned with the biographies of a number of naturalists whose lives were partly and sometimes entirely interwoven with the most striking and scientifically important discoveries in vertebrate paleontology in South America. After a prelude in which the contributions of major and minor figures are briefly sketched (including those of Georges Cuvier and Thomas Jefferson), the story proper begins with Charles Darwin. Simpson then turns to a number of less well known authors, such as Peter Wilhelm Lund, Francisco Javier Muñiz, and the Ameghino brothers, and to paleontologists whose lifespan partly overlapped that of Simpson or who, like John Henry Patterson and Carlos de Paula Couto, were his friends.

The major interest in the book lies in the information it provides about these lesser-known but nevertheless important figures, about whom virtually nothing is written in English. The reader obtains useful and reasonably detailed information, for example, about Carlos Ameghino, the lesser-known brother of the flamboyant Florentino. Moreover, interesting guidelines are provided for the social historian in Simpson's intriguing reference to the persistent vein of nationalism that has characterized disputes about vertebrate fossils, not least epitomized by Florentino Ameghino's claim that his native Argentina was the place of origin of mammals. The book also reflects the growing interest that the English-speaking part of the Americas have in Latin American culture—a phenomenon perhaps indirectly related to the continual increase in the Spanish-speaking citizenry of the United States.

Simpson's treatment of his subject is occasionally anecdotal, and there are traces of the traditional Anglo-Saxon view of the Latin as nice but volatile and quarrelsome. Nonetheless, Simpson's deep knowledge of the subject is apparent, as can be seen from his remark that Florentino Ameghino named too many fossils because of his incapacity to think in terms of populations rather than types.

Simpson clearly shows his likes and dislikes, and, like many old men, he seems to have a special sympathy for those who, like J. B. Hatcher and Lucas Kraglievich, died young before achieving their full potential. Hatcher's biography is particularly vivid; I could visualize the man before actually looking at his picture, which corresponded exactly to my expectation.

Although the book is not scholarly in the traditional sense of the term—and indeed does not set out to be—it is of great interest to active scientists who wish to know more about the development of a branch of their discipline, and even more so to general historians and learned laymen interested in science. It is lively, witty, and good-humored. This is no surprise to those familiar with Simpson's previous books, written by a man who, in the *International Who's Who*, listed among his hobbies and interests: life. How sad that this is the last we shall hear from him.

MARIO A. DIGREGORIO
Darwin College
Cambridge University

BRUCE R. WHEATON. *The Tiger and the Shark: Empirical Roots of Wave-Particle Dualism*. Foreword by THOMAS S. KUHN. New York: Cambridge University Press. 1983. Pp. xxiv, 355. \$39.50.

J. J. Thomson supplied the title of this work when he penned a remark about the thirty-year war between the wave and particle theories of light: "It is like a struggle between a tiger and a shark, each is supreme in his own element, but helpless in that of the other." Bruce R. Wheaton has produced an excellent study of that internal struggle in experimental physics between 1896 and 1926 to understand exactly the nature of matter and light. The discoveries of X-rays and of radioactivity at the beginning of this period inaugurated a long, hard contest to reinterpret theories of chemistry and physics in forms more consonant with burgeoning new experimental evidence. The invention of two independent but equivalent approaches to quantum mechanics at the end of this period brought the theories of atomic structure and radiation into substantial agreement, thus providing a new paradigm for physical science. Wheaton's account is distinguished by its close attention to empirical evidence and by his balanced concern to understand the people and paradoxes involved in this struggle.

In the foreword to this book Thomas S. Kuhn explains why Einstein's *Licht-Quant* theory was largely ignored until French noblemen Maurice de Broglie and Louis de Broglie began in the early 1920s to corroborate both the theoretical and experimental evidence for wave-particle dualism. Wheaton's analyses of the paradoxes of quantity and quality with regard to X-ray energy transfers (pp. 77, 85) lead to an insightful synthesis. He has rediscovered the historical importance of *les frères de Broglie* in establishing that "matter and light are fundamentally one and the same thing" (p. 301). That is the burden of this monograph, and it is very well presented.

Portraits of five of the main actors are carried on the dust jacket: Johannes Stark, Louis de Broglie, William Henry Bragg, J. J. Thomson, and Arnold Sommerfeld. But many other characters also figure prominently in Wheaton's story: Röntgen, Becquerel, Rutherford, Villard, the Curies, C. G. Barkla, Paschen, Dorn, H. A. Lorentz, Wien, Campbell, Max Laue, Bohr, Richardson, Moseley, Millikan, Arthur Compton, Debye, C. D. Ellis, Maurice de Broglie, and M. Brillouin, for example. Significant of the novelty of his interpretation, Wheaton gives relatively short shrift to Einstein, Planck, Philipp Lenard, Max Abraham, Jean Perrin, Friedrich and Knipping, Arthur Schuster, Frederick Soddy, Heisenberg, and Schrödinger. Dirac is not even mentioned, the Compton effect is relegated for explanation to Roger Stuewer's 1975 book, and Manne Siegbahn's Nobel work on X-ray spectroscopy is barely noticed. One of C. T. R. Wilson's cloud chamber photographs of ionization by X-rays is included, but Wm. D. Coolidge's new X-ray tubes developed at General Electric after 1913 also deserve an illustration.

Nevertheless, the symmetry and novelty of Wheaton's narrative are beautiful. Although the subject matter is difficult and presented internalistically, the metaphysical problem addressed—namely, the rejection of mechanistic determinism in favor of indeterministic relations by virtue of two new formalistic approaches to quantum mechanics—is so profound that careful readers will be well rewarded. Perhaps his ignoring so many secondary works about these matters was requisite for this novel thesis.

LOYD S. SWENSON, JR.
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University Park

WALTER H. CONSER, JR. *Church and Confession: Conservative Theologians in Germany, England, and America, 1815–1866*. Macon, Ga.: Mercer University Press. 1984. Pp. viii, 361.

The subjects of this ambitiously conceived comparative study are German neo-Lutherans, Oxford Tractarians, and an admittedly "disparate collection" of Lutherans, members of the Reformed Church, and High Church Episcopalians in the United States. Walter H. Conser, Jr., finds that "confessional theologians" from these groups agreed in espousing a conservative, corporate ideal of both church and society. They rejected the nineteenth-century model of autonomous, laissez-faire man, freed from tradition only, as Conser aptly suggests, to be more efficiently harnessed to the purposes of the national state. The confessionalists preferred organic to atomistic modes of organiza-

tion, and against both pietist subjectivism and liberal rationalism they defended the conception of the church as a divine, universal institution possessed of authoritative truth and potent sacraments. While sharing the Romantic esteem for the personal experience of faith, they insisted that such experience be "objectified" through historically sanctioned creeds. Yet tradition was not necessarily static; in a church flowing like a river through time, as Wilhelm Löhe envisioned it, understanding might broaden with the years.

As often with comparative studies, this book is useful less for its treatment of particular cases than for the comparison itself. Conser is careful to maintain the distinction between parallels and influences. Germany remained an "enigma" for most Tractarians, he remarks, while English theology seemed largely irrelevant to the Germans. American Episcopalians were well aware of John Henry Newman, but a different ecclesiastical environment limited Newman's transatlantic influence. Indeed, although Conser makes a modest case that the three countries he examines were in corresponding stages of concern with industrialization, political emancipation, and national unity, their circumstances seem to have been too dissimilar for extensive mutual influences. That they produced congruent confessional theologies argues, however, for the force of those Protestant traditions and Romantic sensibilities they did share.

Judiciously crafted from solid research, this study nevertheless suffers from deficiencies of proportion. Granted that the author must describe the revivalistic, utilitarian, and other currents with which his protagonists contended, still an inordinate amount of space is devoted to setting stages. Details of Dissenting opposition to mandatory church rates in England provide background deep to the point of obscurity, and the familiar traits of America's Second Great Awakening could be evoked more briefly. Proximity in such matters blurs the focus of what is essentially an intellectual history of theological spokesmen. Conser is more effective when he presents the issues directly, as he does skillfully in comparing the ecclesiastical views of Friedrich Schleiermacher and Friedrich Julius Stahl.

Conser's explanation of the decline of conservative confessionalism after the mid-nineteenth century is somewhat perfunctory and certainly unsatisfying. The corporate vision, he concludes, was simply overwhelmed by "bourgeois individualism," with its attendant forces of industrialism and nationalism. Yet countercurrents of corporatist dissent—idealizing *Gemeinschaft*, the Middle Ages, folk culture, and regionalism—remained strong into the twentieth century, shaping radical and liberal as well as conservative attitudes. It is unclear why confessional theology faded, but perhaps its exponents

were as much harbingers of the traditionalist future as they were vestiges of the traditionalist past.

MICHAEL D. CLARK

University of New Orleans

BENEDICT ANDERSON. *Imagined Communities: Reflections on the Origin and Spread of Nationalism*. London: Verso; distributed by Schocken, New York. 1983. Pp. 160. Cloth \$19.50, paper \$6.50.

"Nationalism," said Albert Einstein, "is an infantile disease. It is the measles of mankind." Benedict Anderson quotes a like metaphor from Tom Nairn: "Nationalism is the pathology of modern developmental history . . . (the equivalent of infantilism for societies)" (pp. 14–15). But Anderson disagrees with Nairn. However dismal nationalism's human sacrifices have been, nations are the nearly universal "imagined communities" that constitute the modern world. In this book Anderson sets out to show how nationalism arose all over the globe and persists today across competing ideological boundaries.

Anderson names three major sequential causes for the rise of nationalism: "print-capitalism," new provincial elites (in the Americas), and the bureaucratic "weld" of nations onto empires (in Britain and Russia). Capitalism's print technology made it possible for people to "imagine" large linked communities that had previously enjoyed no special form of togetherness. Language, always important in creating a sense of community, was not necessarily decisive. Both North and South America generated early models of nationalism derived mainly from the hostility of a colonial elite toward its authenticating center in Europe—Anderson's "metropole." But these early models were altered by new popular nationalists who used language and status to build communities where none had existed (like in Indonesia) and to confirm communities that were otherwise construed (like in China and Vietnam). Finally, the fusion of nation and empire collapsed in the face of a last wave of nationalist imagination after World War II.

To Anderson, nationalism is an anomaly accounted for by neither Marxist nor liberal theory (p. 13). In his comprehensive attempt to explain it, no single world area—not even Southeast Asia, which is Anderson's specialty—receives deep or prolonged treatment. For most countries the evidence comes from a few secondary sources.

Anderson's treatment of Japan illustrates the vulnerability of so general an approach. He categorizes Japan as a society that welded its "official" or bureaucratic nationalism onto a wider empire, and, indeed, so it did. But he overlooks internal Japanese changes before the "abrupt, massive, and menacing" (p. 91) arrival of Westerners in the mid-nineteenth

century. Thereby he misses the earlier rise of Japanese concerns with language and identity in the *kokugaku* movement that gathered force after 1750. The use of additional sources would have enabled Anderson to use Japan as a case of either "old languages, new models" or "official nationalism and imperialism."

This book has the merit of concision, which tends to obviate problems of overgeneralization and limited sources. The result differs sharply from the views of Carleton Hayes, Hans Kohn, and Hugh Seton-Watson. The vocabulary comes from Erich Auerbach, Walter Benjamin, and the Marxists, and the future of socialism is much on the author's mind. Anderson raises large issues, none larger than why nationalism has inspired so many people to lay down their lives in its wars and revolutions. He offers no final answer, but he has produced an original and provocative analysis. His survey persuasively argues that nationalism everywhere substituted new visions of secular glory for older religious and dynastic ambitions. In that sense nationalism is less an "infantile" condition than an outcome of the recent geographical and technological redeployment of human talent.

GEORGE M. WILSON
Indiana University

GREGOR SCHÖLLGEN. *Imperialismus und Gleichgewicht: Deutschland, England und die orientalische Frage, 1871–1914*. Munich: R. Oldenbourg. 1984. Pp. xiv, 501.

This study of imperialism and the balance of power concentrates on Anglo-German relations in the Near and Middle East between 1871 and 1914 and is diplomatic history at its very best. Based on an impressively large variety of primary sources and secondary works, it deals with political and economic issues and takes into account the role of public opinion and personalities. Gregor Schöllgen focuses his attention on the Berlin-Baghdad railroad and problems in the Balkans and provides a fair account of a difficult and complex subject.

Germany's economic penetration of the Ottoman empire at the turn of the century led to considerable friction among the major powers, affected the relationship of the powers outside the Ottoman empire, and contributed to the general tension in the years prior to the outbreak of the First World War. The story has been told many times, and Schöllgen does not uncover any major new facts. He does, however, contribute important details and provide fresh approaches and interpretations. Schöllgen believes, for example, that Germany's successful Middle Eastern policy contributed to the destruction of the European balance of power. He faults German

foreign policy in general, and he criticizes Wilhelm II and Tirpitz in particular for their exaggerated opinion of Germany's power and importance and for their lack of appreciation of the vital interests of other nations (such as Britain) in the Persian Gulf. Despite repeated warnings German policymakers refused to take seriously the possibility of Anglo-French and Anglo-Russian agreements, which, in the end, were brought about by German blunders in North Africa, the Balkans, and the Middle East. As a result, Germany was largely isolated by 1911, a development that German public opinion and German politicians mistakenly interpreted as encirclement by enemies. Schöllgen characterizes German foreign policy in this period as one of improvisation (*Verlegenheit*) and faulty appraisal (*Fehleinschätzung*) and the attitude of Tirpitz and others as divorced from reality (*Wirklichkeitsfremd*). Schöllgen believes that Germany's lack of political and colonial experience was largely to blame.

Germany's global policies, Schöllgen writes, were no different from those of other powers, but they were put forward after the colonial partitions had been completed and in a manner that created public demands (which the government could not possibly satisfy) for the acquisition of colonies. As it became apparent that these colonial promises could not be kept, public pressure on the government mounted. In response German foreign policy increasingly focused on issues of prestige rather than of substance, and, thus, Germany's poor choice of methods, areas, and issues threatened the balance of power. In this unstable situation some German statesmen prepared for and were willing to risk war, although others were ready to adjust to realities to prevent such a disaster.

Despite relying heavily on overly long and complex sentences, Schöllgen has made a valuable contribution to the literature of the period. By analyzing all aspects of the Berlin-Baghdad railroad project, he has demonstrated the weaknesses of Germany's *Weltpolitik* and its consequences for the balance of power.

GEORGE O. KENT
University of Maryland

AKABY NASSIBIAN. *Britain and the Armenian Question, 1915–1923*. New York: St. Martin's or Croom Helm, London. 1984. Pp. 294. \$30.00.

The defeat of Turkey and Germany in 1918 placed Great Britain in control of Constantinople and the Straits, thus ending the Turkish genocide and making restorations in some measure to those Armenians who were still alive. Diplomats and others in the Middle East had been aware of the pitiless massacre of Armenians and the increasing violence

in the 1890s. The remarkable detente and reconciliation of 1908, which saw Turks and Armenians embracing one another in the streets of the capital, did not survive the outbreak of war in 1914. Talaat Pasha was brutally frank when he told Henry Morgenthau that no reconciliation could take place; thus, Talaat condemned the Armenian people to death.

With the armistice of Mudros and the British occupation of the Straits, circumstances appeared to have been reversed. The British strongly favored the establishment of an Armenian homeland, a position supported by those of high standing: Lady Frederick Cavendish; James Bryce; Emily John Robinson, whose works on Armenia were published in 1913 and 1916; and the two Buxton brothers, who published *Travel and Politics in Armenia* (1914).

How could this constellation of brilliance, buttressed by influence in the highest places, have produced ultimate helplessness? What was not expected and hardly could have been was the laxity afforded the Turks by their British conquerors. In describing the reception of the Britons who went through Anatolia disarming the Turks, Winston Churchill emphasized the alacrity with which the Turks obeyed the terms of the armistice. Certainly, the Turks would not have been quiescent forever, but at the time of the Mudros armistice they were quiescent and, in view of their devastating defeat, could have continued so until the Allies needed to effect their future plans.

Such circumstances could not be maintained after the Greek landing of May 1919, which Lord Curzon called the "greatest mistake" made by the Allies. Curzon's assessment is not too strong. The Greek invasion of Anatolia led to the rearming of the Turks—on their own initiative, not that of the peace conference. With the Turks armed and on their home ground, there was no longer any question of dictating to them. The Armenians suffered most, for they alone had nothing to trade. To the extent that Armenia survived at all, it did so because of the Soviet Union. Armenia could hardly have been said to be forgotten since "starving Armenian" became a byword of the Western world, but its effectiveness had come to an end in bloodshed, defeat, and starvation.

How could such a debacle have come to pass? After the British troops left in September 1919, Armenia was literally surrounded by enemies and menaced by a famine in which many perished. The Georgians saved themselves by clinging to the Germans; the Azerbaijanis had oil to sell. Armenia had nothing but a perishing republic, everywhere surrounded by enemies. The people of Britain were ultimately unable to do anything for Armenia because of the geography of Anatolia, the resurgence of Turkish patriotism, Anglo-French rivalry, Sir

Henry Wilson's scorn for Armenia, and the assiduous stress on Muslim opinion. Armenia could have been saved only by Turkish disarmament or assistance from the United States. Neither occurred.

These are thrice-told tales. The justification for reprinting them is in the mass of evidence from previously unpublished sources—reports from the British Foreign Office, the parliamentary papers, minutes of the British-Armenia Committee, the records of the supreme council, and dozens of other sources. Akaby Nassibian has immersed himself in these documents and many more. The time involved is incalculable as the dedication must have been. There is no likelihood that his monumental work will ever be superseded.

JAMES B. GIDNEY
Kent State University

KATHLEEN BURK. *Britain, America, and the Sinews of War, 1914–1918*. Boston: George Allen and Unwin. 1985. Pp. x, 286. \$29.95.

In *American Goes to War* (1938) historian Charles Tansill charged that "unless the American Government took measures to open the lanes of commerce to the Central Powers . . . the United States would fast be converted into a base of war supplies for the Allied Governments" (p. 37). Nearly a half-century later, Kathleen Burk's extraordinary monograph extends far beyond the question of whether the Wilson administration's willingness to supply the British and their allies with ships, arms, munitions, foodstuffs, and finances constituted a monstrous distortion of neutral rights. Written largely from a British perspective and relying heavily on British documentation, her work explores the process by which the British in particular secured vitally important military hardware and other necessities for conducting the Allies' war effort. Most of us who teach and write about the Great War describe the supply side. Burk has succeeded in explaining how the British and American bureaucracies came to terms with this traffic during the period of American neutrality and later during American belligerency.

Soon after hostilities commenced in Europe, the British government recognized the value of appointing a central purchasing agent. J. P. Morgan and Company was retained to coordinate purchasing in the United States and eventually served all members of the Allied coalition. In time Britain acted as wholesaler to her allies, financing their purchases when needed. Only the proud French proved an exception until 1916, provoking the quip from John Maynard Keynes that "Britain had only one ally, France, the rest were mercenaries" (p. 45). Morgan and Company requested private American

loans on behalf of the French as early as August 1914, only to encounter objections when the Wilson administration maintained that such financing would be inconsistent with the spirit of neutrality. By mid-1915 the U.S. Treasury and State departments relented, thereby opening the American money market to belligerent governments. Not surprisingly, many Americans remained reluctant to invest until the Morgan group devised schemes for using British government securities as collateral. To pay for their purchasing needs, the British required at least two hundred million dollars per month.

Following American intervention in April 1917, the British mission to the United States expanded from sixteen hundred to ten thousand persons within eight months. At first, Wilson expressed concern lest the American public get the impression that the British were influencing Washington's policies, while the British worried lest the U.S. war effort siphon off America's valuable assistance needed by the Allies. From time to time, Burk notes, the Wilson administration contemplated the application of leverage to gain concessions from the British, but she fails to explain why such intentions never materialized. Surely, one significant feature of wartime British-American collaboration had to be the British government's circumvention of its embassy in Washington and its ambassador, Sir Cecil Spring Rice, whose ineffectiveness in developing close relations with American leaders was widely acknowledged. Special agents like Sir William Wiseman and successive British missions led by Balfour, Northcliffe, and Reading were intended to meet this need. What seems most impressive to this reviewer is the extent to which these missions succeeded, not their inadequacies.

World War I culminated in the defeat of imperial Germany, the dismemberment of the venerable Austro-Hungarian empire, and revolutions in Russia, but less obvious, if no less significant for twentieth-century international relations, were the consequences for the victorious Allied and associated coalition. Commentators have long observed that the world war contributed to the decline of British power and influence in world affairs. Burk here provides an important explanation for this decline.

LAWRENCE E. GELFAND
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WM. ROGER LOUIS. *The British Empire in the Middle East, 1945-1951: Arab Nationalism, the United States, and Postwar Imperialism*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xvii, 803. \$55.00.

Wm. Roger Louis's comprehensive examination of the British Labour government's disengagement

from the Middle East in the aftermath of World War II is the most impressive diplomatic history written about that region in the postwar period. Louis's work is structured around Ernest Bevin's unsuccessful effort to replace Britain's formal rule in the Middle East with an informal rule built on "equal" partnership and undergirded by economic and social development. By refraining from direct intervention in the region and conciliating "moderate" nationalists, Louis argues, Bevin hoped to avert extreme nationalism, halt the collapse of the British empire, and maintain Britain as the dominant regional power.

Once American commitment to the Middle East's "Northern Tier" (Greece and Turkey) provided Britain with a shield against the Soviet threat, Bevin's evolving strategy was to cut Britain's losses in Palestine; consolidate its position in Egypt, Iraq, Transjordan, and Cyrenaica; and develop the "British" Middle East, whose resources and strategic value Bevin regarded as a substitute for India. If the loss of India made Egypt more important to the British, the loss of Palestine underscored the lack of viable strategic alternatives to the base at Suez, while the advent of war in Korea seemed to confirm the necessity of continued access to the base (which the British desired for air offensives against the Soviet Union in the event of war).

Mutual interests, however, proved difficult to agree on. Attempts to conciliate Egypt in 1946 were counterproductive; efforts to "conciliate" Iran in the early 1950s were too late. Loss of the Indian Army, moreover, rendered the British incapable of exercising their power of old and conducting an imperial adventure in Iran in 1951. In short, neither conciliation nor intervention seemed to be feasible, and events in both Egypt and Iran paved the way for the debacle at Suez in 1956.

Louis recognizes that resentment over seven decades of British influence was profound, that there may have been no way to accommodate rising nationalism and maintain the kind of influence that Britain wanted. This judgment, however, was not apparent at the time, when the British confronted what they saw as two difficult choices: first, between intervention and tacit support for reaction; second, between weakening their strategic defenses and antagonizing nationalists by continuing to maintain enclaves in the region. Although the dilemmas posed by these choices proved incapable of resolution, the contemporary belief that they could be resolved and that the British were engaged in a heroic struggle, Louis observes, "makes the story all the more fascinating" (p. 50).

A brief review cannot do justice to Louis's command of his material, his mastery of issues, and the clarity of his presentation. His meticulous survey of documentary sources, carefully integrated with re-

cent scholarship, enables him to examine key questions, explore alternative interpretations, and provide reasoned assessments, always thoughtful of the larger contexts that inform them. British withdrawal from Greece, for example, must be seen not only in the context of Britain's worsening economic situation but also in view of Bevin's having learned that the United States was working on a new policy toward Greece, his desire to remove all grounds for legitimate Russian complaint over British deployment in the Balkans, and his sensitivity to criticism in the United Nations at a time when he was trying to convince the Egyptians that the British intended to withdraw their troops from Egypt into the Canal Zone.

Louis has a keen sense of personalities and a firm grasp of the bureaucratic contexts within which those personalities operated. Bevin, Arthur Creech Jones, Reader Bullard, Reginald Leeper, John Troutbeck, William Strang, Arthur Dawe, Francis Shepherd, Michael Wright, and a host of others are the subjects of careful scrutiny. We learn whose insights are representative, whose are unorthodox, and where the tone of Foreign Office minutes is misleading. We gain insight into the role of Oriental counselors in Britain's Middle East embassies and into the biases that influenced the relationships among various bureaucracies such as the Chiefs of Staff, the Foreign Office, and the Colonial Office. Louis is particularly adept at characterizing policy conflicts in the context of the personalities who are party to them. Thus, British policy in the Middle East can be seen as a clash between the "Little Englander" and "imperial" ideas about the Middle East embodied in Clement Attlee and Bevin; the Anglo-American Committee of Inquiry's work can be understood as a clash between its respective British and American chairmen. Sensitive to nuances and to assumptions implicit in differing perceptions of a problem, Louis is always conscious that moral and historical issues are ambiguous and that diametrically opposed conclusions can be drawn from the same evidence by men of good will and intellectual integrity. Nonetheless, he is not afraid to make judgments, and when he does they are both reasonable and fair, particularly on Palestine, where he walks with assurance through interpretive and semantic minefields. Scholars looking for a model of historical diplomatic writing could find no better starting place.

BRUCE R. KUNIHOLM
Duke University

BRUCE D. PORTER. *The USSR in Third World Conflicts: Soviet Arms and Diplomacy in Local Wars, 1945-1980*. New York: Cambridge University Press. 1984. Pp. viii, 248. \$29.95.

The USSR's invasion of Afghanistan has aroused much speculation among government and academic experts about what it portends for Moscow's future behavior in the Third World. Is this first use of Soviet troops in a local conflict an aberration, or does it mark an emergent policy pattern?

Bruce D. Porter's book presents five case studies of Moscow's role in local wars: the Yemeni civil war (1962-69), the Nigerian civil war (1967-70), the Yom Kippur war (1973), the Angola civil war (1973-76), and the Ogaden war (1977-78). Each of the book's five core essays is a lucid presentation of the circumstances of the local conflict itself, the nature of diplomatic relations between Moscow and its client, the course of the deepening Soviet involvement, and the contribution of the American and Chinese factors. Each ends with a concise discussion of the results of Soviet action.

The value of these essays is enhanced by tables on the estimated transfer of Soviet weapons and data about the numbers of Soviet and other Communist advisers. Altogether, this part of the book is a model presentation of pertinent diplomatic and military facts set against the broader regional and global backgrounds. On this level the book is a valuable and excellent source of coherently arranged information.

Some questions arise concerning the larger message of this work, however. The book is based on the assumption that the basic Soviet *modus operandi* and the core of the Communists' success is military power. The author posits at the start that the establishment of Soviet rule in Russia by 1922 "depended more on raw military force than on anything else" and that this "lesson . . . played a major role in shaping [the Soviet leaders'] attitude toward the uses and utility of military power" (p. 7). This one-factor explanation of an extended, complex historical event accounts for the largely deterministic presentation of the issues under examination. The book not only stresses the ever-bolder and ascending scale of Soviet military involvement in the Third World but also tends to argue that Moscow has the knack of choosing the winning side.

The fact that the USSR takes a long time in making up its mind to intervene, or that it has at times backed the wrong party, or gets mired in some murky and thankless situation—all indicating both hesitation about the efficacy of force and a lack of sure success—gets short shrift in this study. For readers interested in a more multifarious and less one-sided analysis, Stephen T. Hosmer and Thomas W. Wolfe's recent book, *Soviet Policy and Practice toward Third World Conflicts*, is recommended.

ELIZABETH K. VALKENIER
Harriman Institute
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PETER KARSTEN *et al.* *Military Threats: A Systematic Historical Analysis of the Determinants of Success.* (Contributions in Military History, number 36.) Westport, Conn.: Greenwood. 1984. Pp. xiii, 166. \$29.95.

This is a careful effort, involving an examination of seventy-seven historical encounters, to increase our understanding of why some international military threats are successful (that is, achieve their ends without war) and others fail. Focusing as it does on only one aspect of a recurrent phenomenon, the book inevitably suffers from a certain narrowness of vision: it has no story line, it does not tell us much (indeed, often next to nothing) about the causes or effects of its selected crises, and it never wanders far from definitional and methodological concerns. Nevertheless, such explorations on the borderland between history and social science are exactly what some historians must carry out if the rest of us are going to be able to prioritize the explanatory factors in our investigations on the basis of more than intuition. Peter Karsten, Peter D. Howell, and Artis Frances Allen have done well on several fronts: they have chosen their examples from diverse eras (only 40 percent are post-1945) and different geographic regions (East as well as West); they have made a substantial effort to come to terms with the historiography in each instance and to exclude those situations that they were not sure were comparable; and they have successfully mastered the measuring and statistical skills that they needed to establish correlations.

Much of what the authors derive from this exercise is not terribly exciting, since (as they note) much social science ultimately comes down to a question of which truisms to accept and which to reject. Yet there are a few surprises and a lesson or two relevant to contemporary foreign relations. Military threats have tended to be "successful," the authors predictably conclude, when the threatening state was reinforced by the international climate of opinion, when the threatener's alliance system was more supportive than that of the target country, when the target's resolve and determination were not high, when the target believed that the threatener's military capabilities were greater than its own, and when the crisis did not affect the basic balance of power. But the authors discover astonishingly little correlation between "clarity of threat communication" and success of the threat or between elaborate displays of force by the threatener and a successful outcome. Even more stunning, when Karsten and his colleagues questioned twelve persons in foreign policy-making positions in the United States government in 1977, they found that these individuals not only possessed little awareness of such "facts" but also tended to reject another of the book's conclusions: that the determination of the target

nation is much more important in shaping the result than the resolution of the threatener. The authors were horrified to realize that even after the Vietnam War American defense analysts devote their primary attention to defining "threatener characteristics" (credibility, resolve, displays of force) and often discount or overlook the target's goals, interests, and commitment. Such habits could lead to disaster, they warn, particularly if Soviet analysts look on American-Soviet relations in a similar fashion and treat us "as opportunists to be controlled by shows of resolve" (p. 111).

Thus, this book has significance for both government and academia and should perform a useful function in making statesmen and scholars more skeptical of their assumptions and more sensitive to the various forces at work. It is not the first or most comprehensive study of coercive diplomacy, a subject that has received increasing attention since the early 1970s, but within its compass the book does offer much more data and a more systematic analysis than we have had before. It is a welcome addition to the literature.

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MARTIN JAY. *Marxism and Totality: The Adventures of a Concept from Lukács to Habermas.* Berkeley and Los Angeles: University of California Press. 1984. Pp. xi, 576. \$29.50.

Marxism and Totality will help solidify Martin Jay's position as a leading practitioner of intellectual history in our country. Learned, up-to-date, thoughtful, and clearly written, the book provides a comprehensive history of Western Marxism from Georg Lukács to the present and places it in the diverse political and intellectual contexts that helped shape it. Readers can turn to Jay's book for reliable and insightful accounts of all the major figures who contributed to the remarkable reshaping of Marxist thinking that began with Lukács and Antonio Gramsci. The chapters on Lucien Goldmann, Jean Paul Sartre, Galvano della Volpe and Lucio Colletti, and Jürgen Habermas deserve to be singled out for their coherent and synthetic portraits. Some will take issue with Jay's specific judgments, no doubt, but they will have to acknowledge the scrupulous fairness with which he recognizes alternatives in the richly informed footnotes.

If one nonetheless feels a twinge of disappointment with this book, it stems from the realization that it succeeds better as a history of Western Marxism than as a history of the idea of totality. The two projects do not quite fuse into a well-wrought whole. The reason for this seems to lie in the book's

unusual combination of tough-mindedness and soft-heartedness. Jay is an advocate of some form of Marxist or Marxist-inspired totality, but a disillusioned one. Recognizing the Protean nature of totality, he distinguishes several forms the notion has taken, both within Marxism and outside it: expressive (the totalizing externalization of an acting subject), longitudinal (the wholeness of history conceived as leading toward a final goal), latitudinal (the unified whole of a layered structure), and normative (the totalization of action in terms of a particular social or moral purpose). Western Marxism began with Lukács's attempt to place totality, in its strongest, expressive form, at the center of Marxist theory, but the movement's subsequent history (reaching a kind of climax in Theodor Adorno) is one of the repeated abandonment or rejection of holism. What remains for the most part is a decentered totality, no longer claiming the same organic integration among its parts as the others. Only with Habermas does the revival and potential reaffirmation of the notion in a strong form become possible. To Jay that revival deserves to be furthered as a necessary response to a world brought together by technology and the possibility of universal destruction. Habermas is therefore introduced as a kind of hero-savior, the person whose combination of intellectual qualities promises to rescue totality from the limbo into which it has been cast by its previous history.

But Habermas's project is blocked too. It rests on the universalization of a rational form of "communicative practice" whose possibility is brought into question by the more constrictive image of language made current by the structuralists and their successors. Jay concludes with the hope that Habermas's position might be strengthened by a dialogue between him and Michel Foucault, the most pertinent representative of the French antitotalists.

Even if Foucault's early death had not intervened to make the impossibility of such a conclusion definitive, its inherent weakness would point to the questionable features of such a historical construction. However sympathetic one may be toward Jay's general views, the hope that Habermas and Foucault together might solve the problem is bound to appear thinly justified against the weight of the problems accumulated for the notion of totality within Marxism by the rest of the book. That Jay appears unaware of this seems linked to his unwillingness to ask certain questions about the forms of totality he distinguishes. Why does Marxism both express itself in this combination of totalities and demand their rejection? Where on the map of totalities should we locate the points of intersection and difference between Marxism and those other theories and practices from which it draws but that it cannot rejoin if its own transformative project is to survive? These questions are often implied in Jay's

discussion, but they are seldom explicitly addressed and never formulated in a way that seeks to unify the separate treatments of individual thinkers around them. The book's structure is itself decentered. Each thinker is assessed for his contribution to the hoped-for adequate and sustainable notion of totality and criticized for what other writers, or Jay himself, see as the difficulties and insufficiencies of his position. These discussions are always intelligent and often enlightening. But the question of why the notion itself has the kind of history it does—that pattern of recurrence and dissolution the book reveals—is never really confronted.

Did that pattern arise from some fundamental tension within Marxism, whose existence underlies and finds expression in its history? Jay's decision not to pose this question seems tied to his desire to keep the history of totality open-ended, so that the hoped-for reconstructed version can be received into it. But the existence of such a tension is strongly suggested by the history he tells. The recurring clash between the recognition that history has no foreordained goal and the persisting idealist impulse in Marxism that was given new impetus by Lukács and Gramsci seems to me to constitute much more of the history of the idea of totality than Jay allows. It can be identified in the career of Marx himself (with unhappy consequences for his own totalizing project, which Jay's discussion of Marx does not consider). It reappeared when Colletti, discovering that Marx's method of analyzing economic reality actually preserved an idealist dialectic of totalization, even while his anti-Hegelian methodology cried out against it, decided to abandon Marxism. The same tension has driven Habermas toward a form of totality that casts elements of Marxist analysis onto a framework that is closer to Kant than to Hegel or Marx. His affirmation of totality is based on a dualistic division between elements of technique and self-regulating "system" on the one hand and the communicative action of rational subjects on the other. It gives so much recognition to the real historical weight of the former that (as Jay's discussion makes clear) the vision of human wholeness in his work recedes to the status of a Kantian regulative idea; totality here loses the claim to structure real historical experience, which any genuinely Marxist theory must preserve.

By identifying the problems in Habermas with the deconstructionist critique, Jay detaches him from the dilemmas that make totality's history a mirror of the recurring tensions within Marxism. Such treatment serves the open-endedness Jay espouses, helping guarantee totality against closure through the experience of its own history. But it also points to one main source of the book's inability to unify the two projects of the history of totality and the history

of Western Marxism. A closer integration of the individual discussions would make the recurring and persisting problems stand out more clearly, making the happy ending less plausible. *Marxism and Totality* is a book to which readers will return for its accounts of the individual thinkers who make up Western Marxism. Perhaps its lesser success in integrating those figures through the concept of totality is a tribute to the difficulties that notion has installed within Marxist theory.

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ANCIENT

T. G. H. JAMES. *Pharaoh's People: Scenes from Life in Imperial Egypt*. Chicago: University of Chicago Press. 1984. Pp. 282. \$20.00.

T. G. H. James's book is a social history that will appeal not merely to the specialist but to the educated layman as well. James attempts to replace the nineteenth-century classic, John Gardner Wilkinson's three-volume *Manners and Customs of the Ancient Egyptians*. In 1837, when Wilkinson published this work, Egyptology was still a young discipline, and scholars could still present a comprehensive picture of ancient Egypt based on monuments, artifacts, and the writings of classical authors. That cannot be done now; the classical authors, with the exception, perhaps, of Herodotus, are no longer considered reliable, but in their place is a great number of documents from Egypt itself that Wilkinson never knew. It is on these that James bases his vignettes.

The first chapter deals with the nature of the documentation. The inscriptions and papyri that have survived are not representative; the bulk of the nonliterary, unofficial documents come from the necropolis of Thebes and belong to a 300-year span—short compared to Egypt's 3000-year dynastic history. Official documents, however, must sometimes be taken with a grain of salt: the lengthy composition of Ramses II on his "victory" at the battle of Qadesh covers up a near defeat, although Ramses might have gotten away with his deception if we did not have Hittite records to check him. But, these caveats notwithstanding, we are remarkably well informed.

The second chapter, on the role of the vizir, or chief minister of the pharaoh, centers on Rekhmire, vizir under Thutmose III, whose tomb has survived, although Rekhmire himself may have fallen from grace before he could use it. The third deals with the rich literature on legal litigation. The fourth chapter examines the life of the farmer, the fifth that of the far more prestigious scribe, and the sixth

that of the craftsmen. The last two chapters explore Egyptian domestic life: how they built their houses and their villages and how they managed their households.

It is not easy for us to understand a society that was old when classical Greece was at its height. But James has successfully given *Pharaoh's People* immediacy by using ancient texts wherever he could, and he has newly translated most of those he uses. Quite apart from the value of this book as social history, it also serves as a reminder of how rich the documentation is now. This is a fine book, worth recommendation to anyone who wants to know what life in ancient Egypt was like. And for those interested in dynasties James appends a brief chronology, but it is not his own: it is borrowed from Jürgen von Beckerath's *Abriss der Geschichte des Alten Ägypten* (1971), and there is nothing controversial about it.

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ALAN E. SAMUEL. *From Athens to Alexandria: Hellenism and Social Goals in Ptolemaic Egypt*. (Studia Hellenistica, number 26.) Louvain, Belgium: Katholieke Universiteit Leuven. 1983. Pp. xi, 130. 600 F.

It is conventional wisdom that the Hellenistic world represented an integration of Greek and alien cultures. Alan E. Samuel's small tome constitutes a major revision of that view. It reveals a lack of interaction between Greek and foreign—especially Egyptian—cultures that will challenge many cherished conceptions about the nature of cultural and administrative innovation during the three centuries following the death of Alexander the Great.

The first part of the book argues that classical Greeks had an economic theory. The evidence from the fourth century B.C. suggests that at least some Greeks (those known to the public at large as well as to the Macedonian ruling classes that would lead the early Hellenistic kingdoms) were aware of demand-price ratios and the relationship between demand-profit shifts and changes in the levels of occupation. For the ancient thinkers the description of practical and theoretical economic activity was part of an integrated body of political, economic, and moral thought in which self-sufficiency, abundance, and stability were the goals. There was no interest in growth or progress, both because growth for its own sake was open-ended (and thus morally suspect) and because it was destabilizing.

Greek society was therefore characterized by activities designed to achieve specific, limited goals, among which economic growth and increased productivity were not included. Whereas little theory in the documents is evident before the fourth century B.C., the persistence in the real world of long-term

regular activity consistent with these goals suggests that a theoretical construct existed.

The remainder of the book demonstrates how the relatively rich documentary materials from Hellenistic Egypt support the idea that classical cultural patterns continued for the next three hundred years. Samuel, an acknowledged expert in papyrology, reviews a wide range of evidence (much of it only recently published) that supports his case. Human activity was little changed from practices in place by the fourth century, not only in the economic realm (which in Egypt was mainly agriculture) but also in mining, metallurgy, ceramics, engineering, and architecture as well as in literature, art, and religion.

Even Michael Rostovtzeff's long-dominant view that Egypt was a society of two classes—with Greeks and Hellenized Egyptians forming the top tier of rulers and administrators and the native population constituting a suppressed lower order—comes under attack. Samuel shows how pervasive was native Egyptian influence even in the higher echelons of Ptolemaic administration and how unaffected Greeks remained by their exposure to it. For example, the corpus of Demotic documents, both literary and administrative, far outweighs that in Greek; as for the Greeks themselves, their preferences in literature remained classical (such as the works of Homer and Euripides) rather than contemporary, despite what we have regarded as a flourishing literary scene at Alexandria. Greek culture remained inherently conservative and unwilling to assimilate many new ideas.

Samuel presents a powerful case. Despite some flaws—inadequate distinction between the polis and the state in his discussion of economic theory, a reluctance to deal with population increase as a destabilizing factor until near the end of the book, a weak section on Hellenistic art, and a superficial link between economics and other aspects of culture—the thesis is a successful challenge to long-held ideas about the dynamism of Greek culture.

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M. I. FINLEY. *Politics in the Ancient World*. New York: Cambridge University Press. 1983. Pp. vii, 152.

This book is a revision of four Wiles Lectures—"State, Clan, and Power," "Politics," "Popular Participation," and "Political Issues and Conflict"—given at the Queens University, Belfast, in May 1980. It also includes two chapters, "Authority and Patronage" and "Ideology," based on the J. C. Jacobsen Memorial Lecture of the Royal Danish Academy of Sciences and Letters (published originally in the academy's *Meddelelser*). The book, though brief, is a

brilliant analysis of the politics of the Greek and Roman city-state. It is based on the observation that politics "has more of the implication of the ways, informal as much as formal, in which government is conducted and governmental decisions are arrived at, and of the accompanying ideology" (p. vii). In addition, M. J. Finley maintains that there is no meaningful distinction between state and government in politics, that a government-state implies power both internally and externally, and that the choice of those who govern and the ways in which they govern depend on the structure of the particular society being scrutinized.

Finley does a beautiful comparative study of the Greek and Roman polis: originally, they both had similar social structures, were governed by "aristocrats," and used the timocratic principle. Both had citizen armies, although the strictness of Roman military discipline was in contrast to the more relaxed Greek discipline. What characterized Athens, Sparta, and Rome was the continuous acceptance of their political institutions and of the men and classes who directed them. In their own ways, in addition to the provision and control of public finance, community patronage (*liturgy* in Athens; "bread and circuses" in Rome) was worked into the system. What Finley points out most clearly and backs up with documented evidence is that both Greece and Rome enjoyed an element of popular participation, and political leaders, no matter how they acquired their positions, were compelled to gain popular support. "There was, in short, a measure of genuine popular participation" (p. 70), even though citizenship was restricted, women were excluded from voting, and slavery existed.

Finley reminds us that the Greeks and Romans invented politics as we know them and that they also invented political history (that is, the history of war and politics). But he is also careful to point out that ancient historians wrote the history of policy, especially "concerning themselves with the *mechanics* of policy-making . . . only in moments of acute conflict turning into Civil War" (p. 54). If the attitudes of the ruling class were exploitative, so was the psychology of the mass of citizenry. As long as the legitimacy of the political hierarchy remained unchallenged and politics were about issues and not persons, politics flourished. When men's feelings of allegiance to the state faltered, politics ceased to be useful to the people, participation came to an end, and politics themselves ceased.

This is an important book about politics in the Greek polis and the Roman republic; it is also an important study of what makes for a workable and popular political process.

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VILLY SØRENSEN. *Seneca: The Humanist at the Court of Nero*. Translated by W. Glyn Jones. Chicago: University of Chicago Press. 1984. Pp. 352. \$25.50.

In 1976 Miriam T. Griffin published *Seneca: A Philosopher in Politics*, a book now universally recognized as the standard historical work on Seneca, and especially on the complex problem posed by the relationship of Seneca's philosophical writings to his political career. In the same year the Danish original of Villy Sørensen's work, another study of Seneca the philosopher and statesman, was also published. The original has apparently made little impression on scholarship, but it is now available through translation to a much wider audience. Sørensen was perhaps unaware of Griffin's work in 1976, but for the present version of his book some acknowledgment of it ought to have been made, if only to convince readers of his credentials and seriousness. As it is, Sørensen remains ignorant of the important contribution made by Griffin. Consequently, his book is a curiosity to which scant attention will be paid by historians of either the principate or Roman thought.

Sørensen's work gives an account of Seneca's life and a description of his philosophical and literary output, including the dramas, one of the few areas in which Griffin's work is deficient. Sørensen's method is to interlace the chronological narrative of the life with summaries of the writings, but objective analysis is not his intention. Rather, he argues the thesis that Seneca discovered the humanity of the individual, a philosophical insight that Sørensen believes to be of great relevance for solving problems in the modern age. That is to say, Sørensen has interpreted Seneca in light of his own philosophical assessment of the contemporary world, and, inevitably, the result is distortion and misrepresentation of the past.

Seneca himself has never lacked admirers, and much in his philosophy excites approbation. But even in his own lifetime he was a controversial figure, a man who feathered his nest while preaching the virtues of restraint and austerity, and criticism of Seneca the hypocrite has a long history. Sørensen cannot pretend otherwise, but he dismisses Seneca's detractors scornfully on the ground that they exercise no more than superficial judgments. That is an ironic conclusion, however, given the basis on which Sørensen's own judgments are formed; for his knowledge of Roman history in general and of Senecan studies in particular is minimal, while his grasp of the technical problems raised by the evidence from which he proceeds, but rarely cites, is tenuous. The chronology of Seneca's writings, for example, presents a set of complex issues that Sørensen scarcely recognizes, and details awkward for an apologist to explain away, such as

those on Seneca's loans to the Britons, are simply omitted from consideration. Moreover, a much deeper examination is required of the political aspects of Seneca's career; because Seneca made statements that may today be considered humane, it cannot be merely assumed that he was therefore responsible for everything "good" that occurred in the reign of Nero. The logical fallacy is patent but unquestioned here. The result of these various shortcomings is that Sørensen's book is riddled with dubious assertions, anachronistic historical interpretations, misprision, and plain errors of fact. The most to be said in its defense is that it offers a simple guide to the contents of Seneca's works (and Sørensen is a better philosopher than historian), but this is hardly a sufficiently redeeming feature. With Griffin at hand, Sørensen is largely superfluous.

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IRFAN SHAHĪD. *Rome and the Arabs: A Prolegomenon to the Study of Byzantium and the Arabs*. Washington, D.C.: Dumbarton Oaks. 1984. Pp. xxxi, 193. \$12.50.

IRFAN SHAHĪD. *Byzantium and the Arabs in the Fourth Century*. Washington, D.C.: Dumbarton Oaks. 1984. Pp. xxiii, 628. \$30.00.

These two books make an important addition to our knowledge and understanding of Rome's and Byzantium's relations with the Arabs during the seven hundred years that elapsed between Pompey's establishment of Roman domination in Syria and Palestine in 64 B.C. and the battle of Yarmak in A.D. 634. They will be followed by two later volumes that will chronicle the relations between the Arabs and the eastern Roman world during the fifth century and the years of the sixth and seventh just prior to the Islamic conquest of most of the Near East.

Rome and the Arabs, the first and briefer volume, tells the story of the first three centuries of relations between Rome and the Arabs to the destruction of Palmyra by Emperor Aurelian in A.D. 272. Irfan Shahīd begins with the clearly expressed premise that by the first century B.C. Arabs were established as rulers and inhabitants of a broad band of former Seleucid and Ptolemaic territories in northern Syria, Transjordan, and southern Palestine, as well as in Egypt between the Nile and the Red Sea. These Arabs were without any overall political unity and were affected in varying degrees by both Judaism and Hellenism. Some, like the Idumean Herodians of Palestine, were more influenced by these cultural currents than others. For about a century or more Rome used the Arabs as a shield of client kingdoms between their empire and the Iranians to the east and the more nomadic desert Arabs to the south and southeast.

During the remainder of these first three centuries Rome slowly but surely annexed and absorbed these Arab client kingdoms, beginning with that of the Herodians in Flavian times and continuing with that of the Nabateans under the Antonines and Palmyra during the reign of Aurelian. As Rome did so, it was forced to set up a new system of frontier defenses all the way from the Euphrates to the Gulf of Aqaba and the northern part of the Hejaz. And as this happened many of the more sedentary Arabs in these areas became Roman citizens. Some, like the half-Arab Severans and the full-blooded Philip the Arab, even came to wear the imperial purple. Others, like Odenatus and Zenobia of Palmyra late in the third century, were able briefly to take over the entire Near East from Anatolia to Libya, defeat the Sassanians in battle, and defy Roman might with some success.

During this same period Christianity began to spread throughout these Arab-inhabited lands, the king of Edessa being probably the first ruler to accept it for his people and Philip the Arab the first Christian Roman emperor. Although often viewed as heretical by other churchmen throughout the empire, these Arabs had by the reign of Diocletian laid the foundations of both an Arab episcopate and an Arab monasticism that would grow in importance in the years to come.

The next period, covered in *Byzantium and the Arabs in the Fourth Century*, witnessed many changes between the Arabs and the Roman-Byzantine world. Since the shield of Arab client kingdoms had been eliminated once and for all, it was necessary to establish a new defense system. Under Diocletian and especially under the house of Constantine, this came to consist of an extensive system of fortifications, from the Euphrates to the Red Sea, manned not by legionary troops but by Arab *foederati* organized under kings who controlled loose and changing coalitions of tribes. Paid in subsidies of money and *annona*, these *foederati* were able to assist Rome in its wars with the Persians and at times extend Roman interests as far south as Yemen. The first such Arab *foederati* ruler was the Lakhmid Imud' al-Qays, but more important late in the century were the Tankhids who were settled along the Euphrates' *limes* and, as kings, controlled much of this frontier region in the interests of Rome.

This same period also saw such auxiliary military forces well enough trained to be able to fight on equal terms with Sassanian forces during the campaign of Julian against Persia and to defeat Roman imperial contingents during a revolt toward the middle of the century. In short, they were much more than irregular, poorly trained military contingents.

Equally vital was the Christianity—now essentially Orthodox—that spread through this Arab popula-

tion in Roman domains, in the Sassanian realm, and even beyond in Arabia proper. By the fourth century this Christianity was mature enough to have created a well-defined Arab episcopate, Arab centers of monasticism in the Sinai and elsewhere, and a center of Arab Christian culture in Edessa. It also saw the Christian faith spread as far south as Ethiopian Axsum and across the Red Sea into Yemen. The author's claim that this marks the beginning of an Arab Christian period seems more than justified.

Finally, Shahid makes clear that by this time there was an Arabic Christian literature that was as important as the better-known pagan pre-Islamic poetry that the Hejaz and southern Arabia had already developed. This culture, expressed in classical Arabic, was of a richness and importance that helped lead to the cultural flowering of the Umayyad and Abbasid eras.

The author is to be congratulated for producing a new and important view of the Arab world during these centuries and for making extensive use of scattered Byzantine, Syriac, Islamic, and other sources to illustrate his thesis. It is unfortunate that, in doing so, his presentation is often repetitive and confusing. Most thoughtful readers, however, will forgive him his shortcomings in the light of the importance of what he has to say. Others may wish he had broadened the picture even more to make clear how, during these centuries when Christianity was spreading throughout Arabia and its borderlands, Judaism was doing the same as a competitor. And this would help explain not only the Falasha, or black Jews, of Ethiopia but also the large converted Jewish Arab population of Yemen or those in Medina where Muhammad was to flee two centuries later. It would also help explain those Jewish elements that so stand out in the Koran.

ARCHIBALD R. LEWIS

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CLARE STANCLIFFE. *St. Martin and His Hagiographer: History and Miracle in Sulpicius Severus*. (Oxford Historical Monographs.) New York: Oxford University Press. 1983. Pp. xiv, 396. \$45.00.

Early in this century the French scholar E.-C. Babut set forth the thesis that St. Martin of Tours was the creation of Sulpicius Severus to advance his rigorist program. Babut was answered by the Bollandist scholar Hippolyte Delehaye in 1920, yet in some form the questions posed by Babut still determine the agenda for scholarly discussion of the vita S. Martini. The merit of this thorough and learned study is that Clare Stancliffe gives Babut's skepticism a thorough hearing yet skillfully leads the discussion in a more satisfying direction. In her view it was Sulpicius who "created" St. Martin, that is, raised

him from obscurity to holy fame, but it was Martin's unique character that prompted Sulpicius to adopt his ascetic views and led him, with the help of his literary skills, to write the kind of life he did.

The problem with the Martinian writings, however, is not simply one of historical reliability. Sulpicius's portrait of Martin was vigorously attacked by contemporaries who, like Sulpicius, knew him. And it is this second problem that dictates the shape of the book and provides Standcliffe with the evidence to deal with the question of historical credibility. Besides the obligatory chapters on Sulpicius's life and education, the literary form of his various works, and his presentation of Martin, there is a long chapter on the mental world of the hagiographers and a detailed account of the ecclesiastical conflicts of the day, in particular the dispute over rigorous asceticism in the Priscillianist affair. Standcliffe's contribution is to set the work of Sulpicius and the life of his hero securely within the religious and social milieu of late fourth-century Gaul as well as to sketch out the wider context of hagiography in East and West.

Her point is to show that Martin's individuality shines through the *vita*, that the things that are said of him fit the ecclesiastical situation and the spiritual horizons of his time, and, most important, that the *vita* was subject to criticism, not so much because what it claimed was untrue (though some things were) or excessive (though it was) but because Martin had in fact been the subject of criticism during his lifetime for his extreme asceticism and because he had been a soldier before becoming a monk.

This argument requires weighing many complex and intricate historical, chronological, and social questions, and Standcliffe negotiates each topic patiently and judiciously, without losing sight of her overall thesis. Her handling of the historical material is firmer than the discussion of methodological issues, such as magic, miracle, and literary genre. For example, she distinguishes between lives written to remember a concrete individual and lives written to make a religious point. But the literary genre of "lives" was new to Christianity in this period, and most were based on the remembrance the author had of a specific individual or on communal memories of someone who had died recently. They differ from the Lives of Plutarch or the lives written by Philo, intended to illustrate the life of a statesman, and from the appeal to classical or biblical heroes (such as Heracles or Moses) for the purposes of moral edification. Standcliffe's study, however, does not rest on methodological points. It sits solidly on an impressive accumulation of evidence and a strong dose of common sense. Hyperbole was the stuff of ancient rhetoric, and even Gibbon admired Sulpicius's style as "not unworthy of the augustan age." That the *vita* should be filled with exaggera-

tion and apparent artificiality is a mark of *inventio*, not of make-believe.

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MEDIEVAL

THOMAS S. BURNS. *A History of the Ostrogoths*. Bloomington: Indiana University Press. 1984. Pp. xvii, 299. \$19.95.

The Ostrogoths have not lacked commemoration in recent times; Herwig Wolfram's monumental *Geschichte der Gothen* appeared only a few years ago. The poignancy of the Ostrogothic regime in Italy under the beneficent Theodoric—the Indian summer of late Roman culture, brutally ended by Justinian's armies—has attracted much attention. The subject is hardly exhausted, but anyone returning to these well-worn tracks should have something of value to say.

Thomas S. Burns never clearly sets out his real aims. He does not systematically present what is known about the Ostrogoths and seems not to have intended to do so. He eschews a chronological exposition even when it is called for; the results in chapters 4 and 9 are appalling. He discourses on Ostrogothic religion without reference to the *Codex Argenteus*. He sheds no light on Ostrogothic society (least of all its often-mentioned but never-defined nobility), although he claims even to illustrate its metamorphosis. One easily collects examples of gross errors of fact (the Monophysites are said to have resented the Henotikon as "an encroachment . . . into the theology of Chalcedon" [p. 78]), chronological absurdities (the experiences of Paulinus of Pella in the 410s are called "near contemporary" to the Ostrogoths in Italy a century later [p. 125]), mangled Latin (*Codex Theodosiani* [p. 172]), travestied documents (the letters of Ennodius have nothing to do with Gothic settlement [p. 83 nn. 60–61]), misinterpreted quotations (the Procopian story cited on page 203 is at once shown to be misunderstood), and abuses of evidence (because Tacitus devotes one sentence to *Gotones*, Burns applies all the *Germania* to the Goths). Legal texts, including Cassiodorus's *Variae*, are normally treated as though they were documents of *histoire événementielle*. The vaunted use of archaeological material proves arid. From every technical standpoint, including the omission from the notes of the real sources of information (for example, p. 214 n. 47), the book is a disappointment. The sentences plod on and on, rarely joining into coherent paragraphs. What humor there is seems unintentional: "In accordance with [Totila's] commands, Roman women were safe. Despite their

cravings, the Ostrogothic soldiers abstained from rapine, which was so frequently the victor's tax upon the conquered" (p. 210).

Impatient with what has been better said before, Burns is captivated by the hardly new problem of the encounter of (alien) Goth and (civilized) Roman and expresses conviction that vital aspects of this meeting have been missed hitherto. His inner sense may be sound, but messing up the known is a poor springboard toward originality. Lacking either new documentation or analytical finesse, he proceeds instead to invent what was in the heads and hearts of individuals and groups. He is locked into stereotypes and slogans, such as pro-Roman and anti-Goth, that reduce the two nationalities to the coarse terms of sporting clubs and their fans. An early sentence suggests the crudity of the argumentation: "Our anonymous Frankish warrior turned Roman soldier literally came from another world, a world long converging on the highly institutionalized Roman civilization huddled around the Mediterranean Sea" (p. 10). Yet the Frank in question, for all his alleged alterity, called himself *Francus civis*, applying to his milieu one of the cherished catchwords of the ancient Mediterranean.

One reads a book like this with deep regret and reviews it with deeper.

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HELMUT BEUMANN, editor. *Beiträge zur Bildung der französischen Nation im Früh- und Hochmittelalter*. (Nationes: Historische und Philologische Untersuchungen zur Entstehung der Europäischen Nationen im Mittelalter, number 4.) Sigmaringen: Jan Thorbecke. 1983. Pp. 271. DM 128.

Following the tradition of Perry Schramm, Eugen Ewig, and Margret Lügge, the three articles in this book focus on the idea of the nation in Capetian kingship. They are accompanied by Helmut Beumann's fine introduction. In "Kontinuität und Tradition als Grundlage mittelalterlicher Nationsbildung in Frankreich," Joachim Ehlers shows that Capetian kings from Hugh to Louis VI used the legends of St. Denis to legitimize their claims to sacral kingship. As the royal burial site, St. Denis became the counterpart of Reims, the usual place of coronation. Although acknowledging Gabrielle Spiegel's important work on the legends, Ehlers stresses the interpenetration of the imperial and sacral traditions. The Capetians not only maintained a literary link with their Carolingian ancestors through chronicles and epics but also often tried to act out the historical memories as if they were latter-day Charlemagnes. As Paris emerged as the administrative and political center of the Frankish

kingdom after Philip I, the monarchs increasingly centered on St. Denis to retain a continuity with the venerable past, namely, the person of Charlemagne and his kingdom of the Franks.

"Französisches Sonderbewusstsein in der politisch-geographischen Terminologie des 10. Jahrhunderts" by Bernd Schneidmüller treats the shifting interpretations of *Francia* and *Gallia* in two chroniclers. In contrast to Lügge and W. Kienast, Schneidmüller emphasizes the geographical meaning that Flodoard, a tenth-century canon of the cathedral of Reims, attributed to *Gallia*. Flodoard went beyond the usual categories of the kingdom of the Franks or the duchy of *Francia* and exploited the concept of historical Gaul to promote the political pretensions of Reims. Schneidmüller contends that the extravagant claims of Richer of Reims—often ridiculed in modern German historiography—were a continuation of Flodoard's view of Gaul as a political entity and were taken to be as relevant in the tenth century as they were in the days of Julius Caesar. Richer's seemingly incidental allusions to Gaul, Schneidmüller insists, were actually part of a carefully considered justification for Robertian assertions against Rhineland lords. But Richer went further than Flodoard in his arguments for a moral foundation supporting a revived Frankish empire, itself rooted in the ancient territory of Gaul. Richer joined Flodoard's idea of Gaul as a *patria* to the idea of Gaul as the historical area of the Gallic people. Although some readers may feel uneasy about the relatively few references to this *Gallia-Begriff* in the writings of Richer, Schneidmüller offers a fascinating test case of how notions of a people and a geographical region formed the rudiments of the later concept of the nation-state.

Richard Hamann-Mac Lean's careful study, "Die Reimser Denkmale des französischen Königtums im 12. Jahrhundert: Saint-Remi als Grabkirche im frühen und hohen Mittelalter," (which includes 34 illustrations and 239 plates) relates the tomb motifs of Louis IV, Lothar, and Hincmar in the abbey church of St. Remi to the royal iconography in the Reims cathedral and also to other artistic representations of corresponding themes, such as those in manuscript illuminations.

These three essays are complimentary and of exceptionally high quality.

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SUSAN REYNOLDS. *Kingdoms and Communities in Western Europe, 900–1300*. New York: Clarendon Press of Oxford University Press. 1984. Pp. viii, 387. \$39.95.

This overview of lay collective activity in Western Europe suggests that informal group action by com-

munities acting as governing units was common at all levels of society until about 1140. Thereafter, customs were often put into writing, which in itself implied the illegitimacy of the earlier, more ad hoc approach to group participation and representation. But the legal changes between 1140 and 1300 were more apparent than real, for they had a prior basis in widespread customary practice. Such legal categories as "corporation" and "feudal" are for Susan Reynolds more recent inventions without basis in the legal practice of the time. There was less difference in practice between Germanic and Roman law than scholars have claimed. The juridical distinctions that are often drawn between social orders had little basis in the collective practices of the Middle Ages, when free and unfree often attended assemblies and were judged together. It is well known that villages were effective pressure groups by the thirteenth century, but fragmentary evidence shows this to have been the case much earlier. The cities did not represent a new departure, for the Roman law of corporations simply built on existing customary practice by which groups could own property and speak with one voice in dealing with other individuals or groups.

Reynolds finds that the growth of royal power during the central Middle Ages was owed not to the monarch's position as head of a feudal hierarchy but to notions that kingship was a natural form of human organization. Collective activity is most apparent in the guilds, parish associations, villages, neighborhoods, and various urban solidarities at the lower level and in the kingdom at the upper; little feeling of loyalty toward or collective activity in the intermediate ground of the province existed before the thirteenth century. She argues that there was broad agreement on the principles of good government. Conflicts arose over abuses, not as protests against the established order. What seem now to be inconsistent and overlapping jurisdictions bothered no one at the time.

This book is thus a healthy reaction against much of the legalistic web spinning so often found in historians' treatments of local and national government in medieval Europe. But Reynolds's weakness on points of detail has caused her to write a book that will annoy nearly everyone who has written knowledgeably on the topics she covers. Her bibliographical gaps are startling. Her last and longest chapter, "Community of the Realm," contains her most egregious lapses. Her statement that the notion of enforceable feudal rights was a *post factum* argument by lawyers to justify what the kings had already done is wrong. She does not consider the legal and contractual obligations created by the feudal oath. In her zeal to avoid a "feudal" explanation, she even manages to attribute the prestige of the twelfth-century French kings to the reputation

of the schools of Paris (p. 281). Her argument that the concept of nationhood was strong in late thirteenth-century Germany because Alexander of Roes had one is absurd. She makes sweeping generalizations based on very few cases, many of which could be, and have been, interpreted differently. I was bothered by the large number of "might" and "perhaps" clauses that became the basis for conclusions of fact. All historians must be selective with their material, but Reynolds simply omits anything that might upset her argument and even dismisses some topics, such as feudal relations, that might have been used to strengthen it. She has performed a valuable service in stressing the continuity of forms of association and the basis in oral tradition of much of the legal development of the twelfth and thirteenth centuries and in cutting through legal arguments to show how people acted collectively at all levels of society. But one must finally agree with her own candid admission: "It may be that because my subject is collective activity I am exaggerating the faint hints of its existence" (p. 121).

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DOMINIQUE BARTHÉLEMY. *Les deux âges de la seigneurie banale: Pouvoir et société dans la terre des sires de Coucy (milieu XI^e—milieu XIII^e siècle)*. Foreword by PIERRE TOUBERT. (Université de Paris IV, Série Histoire Ancienne et Médiévale, number 12.) Paris: Publications de la Sorbonne, with the cooperation of the Centre National des Lettres, Paris. 1984. Pp. 598. 250 fr.

This is one of those daring books whose authors seek to demonstrate *multum in parvo*. Dominique Barthélemy draws on some four hundred archival records plus a scattering of illuminating but difficult narratives to study the lordship of Coucy from its poorly documented origins in the eleventh century to its attainment of baronial status in the thirteenth. His story is full of human interest, for it begins with the notorious Thomas de Marle and ends with the imposing Enguerran IV, on both of whom the author has penetrating (and, on Enguerran, rather novel) observations. But the characterizations are necessarily more social than personal. Barthélemy argues that the lordship, strictly speaking, dates only from the partition between the heirs of Thomas de Marle in 1132–33; that the establishment of the lineage and the determination of the norms of its power were accomplished by about 1150; and that in the later twelfth century the appearance of village lordships marked a redistribution of elite powers in which the old dependence of knights on the castellans was weakened. The latter shift coincided with demographic-economic *croissance* in the Oise-Ailette

region, which is examined with reference to the privileges of Laon, Marle, Vervins, and La Fère. By the thirteenth century the heir to the greater castellanies had become the *Sire de Coucy*; his power over roads and forests was better defined; and his control of castles had been reorganized through an incipient administrative personnel, while dependent village lords were allowed lesser jurisdictional powers. Moreover, the *sire*, having sought by marriage a comital title he hardly needed, had become a baron of France, and by no means the least. The famous judicial confrontation with Louis IX in 1259, Barthélemy suggests, showed the astonishing power of a peerage rooted in lineage: "On voit très concrètement ce que vaut le Sire de Coucy" (p. 481).

His lordship is, in fact, "a little kingdom." But its history is not simply that of France writ small, for the Capetian monarchy Barthélemy evokes *en passant* has yet to find its historian. Rather, this is a history in which only local scenes can adequately describe the mechanisms of power and social adjustment that monarchies of whatever size had in common. Imagining a world of clienteles, individual submissions, and alliances, Barthélemy minimizes the continuity of regalian order and class alike, the former notion being quite as easily adopted by ambitious (and lucky) castellans as by the kings. The meaning of social vocabulary is relative to circumstance or function, which changed in the twelfth century. Feudalism means nothing here, feudalizing everything; and it is among the conspicuous merits of this book that it demonstrates brilliantly how the need to redefine local power in terms of vassallic dependence toward 1180–1200 makes nonsense of efforts to describe medieval institutional change without reference to vassals or fiefs. But like all else the understanding of (medieval) feudal realities changes. The echo of Marc Bloch's two feudal ages in Barthélemy's main title is revisionist as well as pointed: it is the *seigneurie banale* that passed from one age to another about 1180 (the author's Bloch having been updated by Georges Duby), transforming a fluid military elite into an aristocracy of ranks.

Space is lacking to do justice to the illuminating discussions of major points. The geographical incidence of local measures is taken plausibly to be a test of the scope of commercial zones. The apparent decline of the *laudatio parentum* in the thirteenth century is shown to be an unreliable test of familial solidarity. Since acts of individual volition proliferated after 1200, one might better speak of "the intrusion of a new sense of accountability and of juridical rules" (p. 210). Here and there the author is carried away by his evidence. Some laborious pages on the charters of customs and on relations with churches seem to betray the dissertation from which the book is hewn, and these are not the only places where the author's taut and complex thought

makes hard reading. Too little is said about "administration," about changing attitudes toward domains that must be a prime test of the quality of banal lordship. And it is perhaps special pleading to suggest (p. 493) that change is more important than origins in the story of Coucy: were Laon's early archives on the scale of Cluny's, a different story might be told—and possibly one more sympathetic to a notion of change by generation than is entertained here.

What impresses above all is the author's powerful sense of the problematic nature of social order in times of change and of the complication for our grasp of contemporary reality that economic and demographic growth were accompanied by the diffusion of writing and of scholastic law. For Coucy, Marle, and La Fère the conjuncture of the later twelfth century is more complex than for the Mâconnais (or for Berry, in some ways a more comparable region). To succeed in the France of Philip Augustus one had to be something less than a *princeps*. But not much less. This book is not to be mistaken for local history. It will take its just place among the more penetrating recent studies of land and people in the provinces of medieval France.

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R. W. HUNT. *The Schools and the Cloister: The Life and Writings of Alexander Nequam, 1157–1217*. Revised by MARGARET GIBSON. New York: Clarendon Press of Oxford University Press. 1984. Pp. xiii, 163. \$39.95.

This compact and learned book originated as R. W. Hunt's 1936 Oxford thesis under the title *Alexander Nequam*. It gestated for almost fifty years while Hunt's distinguished career as keeper of Western manuscripts in the Bodleian Library and as lecturer in paleography and the transmission of classical Latin texts at Oxford left him scant time to prepare it for publication. Over the years, however, he continued to collect notes and new information that he hoped eventually to incorporate into a published version of his work. Margaret Gibson has carefully completed the job and given us what Beryl Smalley calls in her preface "a monument to the scholarship of the 1930s and a memorial to the author" (p. viii).

Schools and cloisters appear only marginally in the book; the focus remains on Nequam. After examining in the opening chapters Nequam's life (he is said to have been born on the same night as Richard I; his mother nursed the king-to-be on her right breast and Alexander on her left) and the authenticity of his works, Hunt discusses various aspects of the latter. Nequam's talents as a grammarian, his use and knowledge of the classics, and his

skill as a poet, scientist, preacher, commentator, and theologian are examined serially before a brief last chapter on the *fortuna* of Nequam's works. Two very useful appendixes, one listing Nequam's works (complete with references to manuscripts, editions, studies, and *dubia et spuria*) and another supplementing and correcting J.-B. Schneyer's *Repertorium* entry on Nequam, conclude the book.

Hunt's study is very much a monument to the scholarship of the 1930s. It is unaffected by the recent work on grammar, the field on which Nequam made his greatest impact to judge from the manuscripts. Our understanding of Nequam's position on the cusp between the age of Bernard of Clairvaux and Anselm of Canterbury and that of the thirteenth-century scholastic theologians would have been enriched by reference to discussions of monastic and scholastic theology. A complete revision, however, would have entailed a new book. What we have is valuable enough. Hunt's synthetic view of Nequam's accomplishment is based on thorough familiarity with his subject's works, many of which remain in manuscript. Copious citations from the manuscripts coupled with Hunt's own trenchant observations point the way for future theses, studies, and, one hopes, editions. If presentation of Hunt's work to a wider audience stimulates greater attention to the interesting and important figure of Nequam, this book will be a fitting memorial indeed.

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RICHARD KIECKHEFER. *Unquiet Souls: Fourteenth-Century Saints and Their Religious Milieu*. Chicago: University of Chicago Press. 1984. Pp. viii, 238. \$24.95.

In this phenomenology of fourteenth-century sainthood Richard Kieckhefer identifies the theological vision implicit in late medieval hagiography and sets it in the context of the religious culture of the period. The book begins with a reminder of the special characteristics of the materials of hagiography (including vitae, processes of canonization, and spiritual autobiographies); these sources reveal less about the "real lives" of individuals than about their authors' notions of sanctity. This is a study of religious mentalities, and Kieckhefer distinguishes clearly between his own work and that of André Vauchez (on ecclesiastical history and the politics of canonization) and of Donald Weinstein and Rudolph M. Bell (on the sociology of sanctity). *Unquiet Souls* is primarily a study of the "theological assumptions of the sources" (p. 20).

The three individuals portrayed in detail—Dorothy of Montau, Peter of Luxembourg, and Clare Gambacorta—represent, as well as any three

persons could, the immense variety of background and experience among fourteenth-century saints. From the hagiographic materials for more than fifty men and women, Kieckhefer draws several persistent themes. One is the virtue of patience: holy persons were renowned for the intensity and duration of their physical and emotional sufferings. A related theme is devotion to the Passion of Christ. The saints went to great lengths to participate in Christ's agonies—a form of piety almost incomprehensible to modern readers, for whom the saints' delight in illness, wounds, and persecution is masochistic, or worse. Kieckhefer does not apologize for his subjects' "extremism" but places it within a theological system and a religious atmosphere. When Jane Mary of Maillé pierced her head with a thorn during Easter Week and Henry Suso carved "IHS" on his chest, they acted out of a "mystical sense of identification with the Savior" (p. 119).

Sharing the grief of Mary and the disciples at the foot of the Cross, the saints assumed heavy burdens of guilt for the human sins that made the Redemption necessary. The most trivial offenses drove them to confession and penitence, and Kieckhefer notes that scruples flourished in the inevitable gulf between the imitation of Christ and human realities. He also notes that many of these men and women came from "a sort of border zone . . . between the laity and the clergy" (p. 193). Their inner lives were tense; their external lives, fraught with difficulty. They attempted to live beyond the norms applicable to ordinary good Christians, subjecting themselves to monastic standards without the traditional support and discipline of the cloister. It is not surprising that many of them required repeated assurance from sympathetic persons invested with the authority of the institutional church.

This is a splendid book, thoughtful and intriguing, based on profound knowledge of the complex sources of late medieval hagiography. Kieckhefer presents the theological vision of the vitae with a richness of detail that illuminates the experience of these remarkable people, claiming our sympathy for their restless spirits and our attention to the context of their extraordinary lives.

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GEORGE CHRISTOS SOULIS. *The Serbs and Byzantium during the Reign of Tsar Stephen Dušan (1331–1355) and His Successors*. Washington, D.C.: Dumbarton Oaks. 1984. Pp. xxvi, 353. \$15.00.

The reign of Byzantium's Paleologi dynasty between 1261 and the fall of Constantinople in 1453 formed the stage for a prolonged drama of transition from one age to another. Three major protagonists con-

fronted one another in Southeastern Europe and Asia. The first was Byzantium, ravaged by civil wars. The second was Serbia, which was expanding into neighboring regions, leaving but marginal territories to Constantinople's authority. The Ottoman Turks, who were bringing the entire eastern core of Byzantium under Islam, were the third force.

George Christos Soulis unravels the sequence of events in this chaotic, relatively little understood, and crucial period, which is presented with clarity in this tour de force of research and organization. This may explain the structure of this work, in which the main text covers less than one-half of the volume, with the remaining space being allotted to extensive and informative footnotes, useful maps and dynastic tables, and rich bibliographic materials. This is a major contribution to the understanding of events that prevented either a consolidation of Byzantium and its survival or Serbian succession and the continuity of a variant of the Byzantine civilization. The basic motives and forces beneath the events, however, are analyzed marginally and mainly by implication, whether in regard to Byzantium's civil wars or the rise of Serbia. Conquest, as a prime motive and goal, can be attributed at this time only to the Ottoman protagonist.

Expanding at the expense of the aging, dysfunctional Byzantine empire, Serbia had a considerable degree of affinity to it, as seen in Serbian political institutions, law, art, and theology. But the Serbian national variants were clear. The role of the *sabor* limited the monarch's authority, and the social strata represented in the *sabor* extended far beyond those of the feudal magnates, high clergy, and the burghers. Most significant, the Serbian church had been autocephalous since 1219. The use of the vernacular, the development of a system of education, and the role of patron of arts and cultural catalyst, made the Serbian church the binding factor of national unity, and Byzantium and the Patriarchate of Constantinople saw this autonomy as a major threat to their supremacy.

Either in an act of desperation caused by interminable civil wars or as extreme and reckless opportunism, John Cantacuzenus brought the Turks across the Dardanelles to fight as mercenaries against the Serbs. Within months, Sultan Orkhan's mercenaries became conquerors; within a decade, they were able to transfer their capital from Bursa to Adrianople, separating Serbia and Byzantium; within a century, the Turks terminated the vestiges of the age of Rome by taking Constantinople. Dušan was given the papal appointment as *Capitaneus Contra Turchos* in February 1355, placing him at the head of a crusade including Western as well as Eastern forces. The author's view that Dušan's "lack of seriousness" explains why the crusade did not

come about is difficult to accept, since Dušan died suddenly in December 1355 while on campaign.

The relations between Serbia and Byzantium under Dušan's successors could not be of significant consequence. In a defensive effort Serbia was joined by its western neighbors, Bosnia and Hungary, in 1364; at the Maritsa River battle, they were defeated, with the heavily armored nobility drowning and King Louis the Great of Hungary barely managing to escape. A major force of Serbian magnates, acting as independent allies, was defeated again in the second Maritsa River confrontation; and, finally, an international allied army assembled by Prince Lazar Hrebeljanović confronted Sultan Murat in June 1389, in the battle of Kosovo. The Turks prevailed, and a cover of darkness, stagnation, and oppression began to descend, even before the last act of Byzantium's suicide was played out in 1453.

DRAGOS KOSTICH

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HENRYK PASZKIEWICZ. *The Rise of Moscow's Power*. Translated by P. S. FALLA. (East European Monographs, number 145.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1983. Pp. 530. \$32.50.

This work is basically a survey of the history of the peoples of the European Russian plain from primordial times to 1380. Henryk Paszkiewicz uncompromisingly insists that the leading force in what he calls the "Rus'ian" state was the Norse Varangians. They maintained consciousness of their identity for a surprisingly long time, first in Kiev and then in what the author terms the Upper Volga region. In this region the Varangians established uneasy dominion over many Finno-Ugric peoples known collectively as *Merya* to create an entity known as *Vladimir Rus*.

Within this framework the author reviews many familiar faces. Chronicling the beginnings of Moscow's ascendancy, he rejects geographical and economic causations and claims instead that the city's rise resulted from the balance of forces and relationships existing in the ruling dynasty (pp. 198–99). As this statement indicates, the author wants to tell his story in purely political terms, focusing on what representatives of the ruling house and leading families did or failed to do.

Among the points Paszkiewicz makes are the overlooked importance of Pereiaslavl', the need to adhere closely to extant sources and to avoid the overly critical attitude modern commentators have adopted, the significance of dynastic marriages, and the decisive role the Tatars played in the Rus'ian state (Ivan Kalita had less room for maneuver than has usually been thought).

The notes to the text following each chapter are thorough, and chronicles are often quoted in the original. Paszkiewicz, who died in late 1979 before he could revise his book, knew the sources and the necessary languages. The bibliography runs from page 457 to page 490. Five appendixes (which should have been worked into the text), two genealogical tables, two maps, and indexes of sources, authors and editors, personal names, and ethnic and geographical names round out the book.

P. S. Falla's translation reads smoothly; misprints are comparatively few. Paszkiewicz has written a useful survey, although a bit old-fashioned. It adds little to an oft-told tale. His book is not likely to supersede A. E. Presniakov's seminal study or the recent works by J. L. I. Fennell. This seems an unexceptionable end to the career of a scholar who was considered a fierce revisionist on publication of his *The Origin of Russia* in 1954.

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M. B. SVERDLOV. *Genesis i struktura feodal'nogo obshchestva v drevnei Rusi* [The Genesis and Structure of Feudal Society in Ancient Rus]. Leningrad: Nauka. 1983. Pp. 237. 2 r.

Since the late 1920s the official Soviet viewpoint has been that feudalism existed in Russia from the ninth century until the emancipation of the serfs in 1861. Although this approach may not create too many problems for those who focus on the period after the mid-fifteenth century, the extent and nature of feudalism in the pre-Mongol or Kievan period (ca. 850–1240) continue to cause serious difficulties for Soviet medievalists. There is simply precious little evidence from this period, and those primary sources that do exist tend to be vague or uninformative regarding the prevailing socioeconomic system. Consequently, over the years some Soviet historians have voiced certain reservations about the feudalism of Kievan Rus. In response, proponents of the official viewpoint have criticized these skeptics and vigorously defended the existence of feudalism. M. B. Sverdlov's book is basically the most recent defense of the official interpretation of feudalism and is designed to rebut recent doubters, such as I. Ia. Froianov, who have raised questions about the nature and extent of feudalism in the Kievan period.

The book is divided into five sections. The first examines the development of an early feudal class structure in Rus during the ninth and tenth centuries. The second section focuses on the dissolution of the commune and the emergence of the nuclear family. Section 3 explores how the growth of a

developed feudal structure from the eleventh to the early thirteenth century produced a variety of dependent classes. The fourth section analyzes the various groups that formed the ruling class of Rus during these later centuries. The author summarizes his main conclusions in the final section.

Sverdlov follows the official interpretation of feudalism. He equates feudalism with manorialism and tells us that it is a mistake to define the growth of feudalism by the presence or absence of vassal relationships and a system of fiefs and benefices such as existed in medieval Western Europe (p. 228). Sverdlov equates taxation with exploitation so that every tax-paying peasant becomes a member of a dependent, feudally exploited group. The author tries to skirt the crucial issue of serfdom in Kievan Rus by arguing that the entire peasantry belonged to various dependent and exploited groups. When discussing those peasants who truly lost their freedom, however, Sverdlov is aware that too many slaves might suggest a slaveholding rather than a feudal society. Consequently, *kholops* and *cheliad'* are seen as dependent people without rights rather than as slaves. In any event, Sverdlov fails to show the existence of serfdom in Kievan Rus.

I have serious reservations about applying the term "feudal" to any medieval society where there was no system of vassalage based on the fief and no evidence of widespread enserfment of the peasantry. Thus, despite the author's useful insights on certain specific issues and his unquestioned erudition, I find his basic argument unconvincing. He has not demonstrated that the society of pre-Mongol Rus could or should be called feudal.

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MODERN EUROPE

SUZANNE SELINGER. *Calvin against Himself: An Inquiry in Intellectual History*. Hamden, Conn.: Archon. 1984. Pp. 238. \$29.50.

This is a daring book on two counts. First, Suzanne Selinger juxtaposes a variety of cultural developments set in an equally various group of interpretations. Second, she depends almost entirely on secondary material to undergird her thesis. This could mean that she selected secondary works because they support her thesis, or it could mean that her thesis developed out of these secondary materials. In neither case is the result one on which scholars could repose great confidence.

Selinger proposes to examine Calvin's intellect and psyche and then to trace the Calvinist heritage into the seventeenth century. She finds Calvin psy-

chologically to be a thoroughgoing dualist. Intellectually, Calvin's dualism is only slightly modified by the Christian doctrine of incarnation. "Calvin's Luther," she argues, is created by changing Luther's monism to Calvin's dualism while leaving the theological superstructure basically intact.

After an extensive comparison of Luther and Calvin, the author delves into Calvin's psyche via Freud. Selinger's "young man Calvin" is less convincing than Erik Erikson's "young man Luther." I do not believe that anyone can know how Calvin felt as a child when his mother died; Calvin does not tell us, and here Freudian analysis is pure conjecture.

Half the volume is dedicated to an examination of rhetoric and "Mannerism." Whereas rhetoric is appropriate, it is difficult to see why the "Mannerism" of Florence should provide clues for interpreting Calvin or even Calvinism. Certainly, supporting the analysis of theology by an understanding of culture is laudible. When it is done, however, it needs to be done with great care.

The book includes a number of historical and theological oversimplifications and, in some instances, errors. There is not space to mention all of them, but some cry out for correction. Medieval mystics did not seek to obliterate individual personality, nor did "nothingness" mean "a leaving behind of all God's historical means of grace" (p. 28). Calvin did not oppose human free will and goodness to God's in so absolute a sense as Selinger asserts (p. 58). Calvin said that before the fall Adam possessed free will and goodness as gifts, indeed, as part of the image of the creator in the created. Selinger reveals a Lutheran bias with regard to Calvin's Christology (pp. 58, 63–65). On page 64 she seems not to know that orthodox Christian Trinitarian theology asserts that in all works *ad extra* all three Persons of the Trinity are involved so that the Word always works with the Spirit. Indeed, most of pages 58–72 are flawed in similar fashion, as are pages 109–10 and page 118, where Lutheran doctrine on the *communicatio idiomatum* is taken as orthodox and Calvin's doctrine, which is true to Chalcedon, is interpreted as dualist, Nestorian, and ultimately "rooted in Calvin's [psychological] aversion to the mingling of heaven and earth" (p. 119).

This book may stimulate new discussions on Calvin and his times, but it will not withstand careful, scholarly scrutiny. It is also difficult to understand why the author (along with the publishers) remains insensitive to sexist language.

JILL RAITT
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MARY FULBROOK. *Piety and Politics: Religion and the Rise of Absolutism in England, Württemberg, and Prussia*. New York: Cambridge University Press. 1983. Pp. viii, 215. Cloth \$34.50, paper \$12.95.

This is a work of historical sociology, as Mary Fulbrook claims. On the one hand, it is an exceedingly careful and solid study of the Puritan and Pietist movements in England, Brandenburg-Prussia, and Württemberg. On the other, it is a theoretical discussion of the contexts in which this type of Protestantism impinged on the trend toward absolutism. It ends with a formula (on page 181) designed to depict in general terms the relationship between precisionist religious movements and absolutism.

The schematic nature of the inquiry explains why Fulbrook surveys the earlier phase of Puritan history in England up to 1640 rather than the Dissent and Methodism contemporary with German Pietism: in each case, in the period selected the precisionist movements had "their greatest impact on the attempts to institute absolute rule" (p. 11). In early Stuart England the religious movement played an infinitely bigger part in successful resistance to a would-be autocrat than in Württemberg and played a quite different role than the Pietists who accepted a partnership with the Hohenzollern kings in the first half of the eighteenth century. Indeed, the extraordinarily diverse fortunes of the precisionists in the three states leads Fulbrook, in spite of the vocabulary she uses, to be wary of rigidly imposing a structural analysis on the material. A more practical point to emerge from her study is the importance of toleration. In Württemberg, with its remarkable mangle-mangle of authorities, the *Kirchenrat* enjoyed revenues and independence to an unusual extent and, impervious to the ruler and the estates, had on balance little difficulty in both tolerating and supervising Pietist innovations. Some Pietists stiffened the resolve of the estates and estates' committees in resisting the attempts of Friedrich Karl and Eberhard Ludwig to extend the ducal powers, but the great majority had no reason not to keep aloof from politics. Most of the time the government in Württemberg left them in peace to be otherworldly, whereas in Brandenburg-Prussia the hostility of Lutheran clergy and nobles disposed the Pietists to accept the tolerant embrace of the Hohenzollerns, who had long since distanced themselves from the Lutheran interest. In England too, in spite of Elizabeth I, the church of the Elizabethan and Jacobean settlement embraced many elements of the Puritan tradition. Only the Arminian movement, adopted and adapted by Charles I while he appeared to be claiming greater general power in the state, brought the Puritans by degrees to political militancy of a kind unparalleled in Germany.

Fulbrook's exposition moves skillfully along from one part to another of the tripartite topic; there is little distortion to fit the theoretical argument; and much modern German and English research is summarized for the reader. Yet another opus ought

one day to explore whether the political significance of this "hotter sort" of Protestantism would be differently appraised if its influence in Prussia and Württemberg were compared with the role of Disraeli during the attempt at princely absolutism in England in the late seventeenth century rather than fifty years earlier.

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Oxford University

TERRELL CARVER. *Marx and Engels: The Intellectual Relationship*. Bloomington: Indiana University Press. 1983. Pp. xv, 172. \$22.50.

In his introduction Terrell Carver asserts that "the views of the later Engels have *in fact* come to obscure the tenets and indeed the importance of Marx's admittedly difficult critique of political economy" (p. xv). He repeats the argument at various points and concludes that Engels took on "an all-powerful role as Marx's literary executor, political heir and apparently authoritative interpreter" (p. 151). With these statements the author attributes to Marx's writings, despite their greatness, an opacity that prevents his readers, despite their recognition of that greatness, from understanding the unmediated Marx.

The Marx-Engels intellectual relationship, Carver insists, was virtually nonexistent after their early work together and "was very much a story of what they accomplished independently" (p. 151). Yet the friends discussed theory and praxis intensely and closely in their frequent and frequently long letters, and Engels—before contributing immense labors to the two posthumous volumes—gave energetic editorial help in preparing the first volume of *Capital* for publication. When Marx finished correcting the last proofs of volume 1, he wrote Engels an exultant note dated 2 A.M., August 16, 1867: "I have you alone to thank that it was possible!" Reading the Marx-Engels correspondence with skillful selectivity, Carver makes no effort to challenge the evidences of close collaboration; he makes no reference to them and at one point flatly denies the collaboration (p. 129).

Carver's argument is based on his claim of the fourth chapter, "The Invention of Dialectics," that Engels's reinterpretation of Marx began with a review he wrote of Marx's *Critique of Political Economy* in 1859 and was reinforced in Engels's *Anti-Dühring* and other late works. According to Carver, Engels "re-Hegelianized" Marx's materialism by creating a false dialectic and dichotomizing the relationship of matter and consciousness. As one example of the many distortions he wrought, Engels canceled out Marx's qualification that "social being did not exclude ideas (used in practice), and consciousness

(i.e., mere ideas) did not exclude a connection . . . with practical activities" (p. 107). But Carver cannot show how Engels's obscure review of 1859 and his once widely read but since widely ignored writings succeeded in changing what readers understood of Marx's ideas.

Carver's statements suggest the deconstruction method, with its close analysis of selected texts, practiced by the late Michel Foucault. Carver does not directly discuss economy, polity, and society but only such mediating abstractions as dialectics, materialism, idealism, and consciousness as found in given writings of Marx and Engels.

Yet Carver's dialectical exercise merits admiration. Refusing to be deterred by the obdurate materials and writing with precision and economy, he has had the daring to match his man Engels, who possessed a prodigious talent for the eclectic and derivative, with a colossal original genius. He has produced a perfect verbal image of the ineffable and invisible.

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STEPHEN BANN. *The Clothing of Clio: A Study of the Representation of History in Nineteenth-Century Britain and France*. New York: Cambridge University Press. 1984. Pp. xii, 196. \$37.50.

As the subtitle indicates, this complex work by Stephen Bann deals with the representation of the past through various media. It is a study in "'arthrology'—the term coined by Michel Foucault for a science of the 'joints' between forms of discourse within an overall epistemological configuration" (p. 3). Bann expects the joints to lie a little below the surface, and to "penetrate the outer layer of discourse" he employs terms (such as metonymy, metaphor, synecdoche, chiasmus, and catachresis) that are derived from linguistic and rhetorical analysis. His examples range from Leopold von Ranke to taxidermy, Sir Walter Scott's "huge baronial pile" at Abbotsford to Louis Daguerre's "diorama." He treats the historical novel from Scott through William Thackeray to Henry James and historiography from Prosper de Barante through Augustin Thierry to Jules Michelet.

An illustration of the author's technique is the way in which he interprets the two collections that eventually came to form the Musée de Cluny, the history of which provides "an ideal source for tracing the development of historical discourse and the historical sense in the Romantic epoch" (p. 78). Alexandre Lenoir's innovation was to assemble his collection in

various rooms, each of which was devoted to a different century. Specimens represented "metonymically the greater whole from which [they] had been detached" (p. 84). Yet Lenoir put modern busts of historical figures in his "century" rooms and even supplied "marble debris" to the sculptors making them, never sensing, as would a later age, that the "debris" would be more important than any archaistic bust that a modern sculptor might create from it. Alexandre du Sommerard's collection was ordered according to different principles. Sommerard used synecdoche in his arrangement, that is, the objects related to one another and to an outside referent, "the mythic system of 'lived' history" (p. 88). Both precious and utilitarian objects were assembled in rooms designated for a particular function: religious objects were put in the chapel, furniture in the chamber, and so forth. These two collections, "in their sharp opposition[,] suggest . . . an 'epistemological break' of the type formulated by Foucault, and identified by Hayden White as the substitution of one trope for another." The author, who is serious about this analysis, speaks of the "rhetoric of the museum" (p. 88).

The book, which derives much from the wide-ranging Roland Barthes, ends not only with a discussion of some products of the *Annales* school but also with an analysis of several films, including *The Return of Martin Guerre*. Clio's Regency clothing has been abandoned, and the author seeks to know what garb the muse would now put on; that is, what sort of "historical poetics" is appropriate to our epistemological epoch. Bann suggests that Natalie Zemon Davis has provided us with an alternate written account of the Martin Guerre story that is more like history than is the film, which takes the form of a literary composition. (In the words of Barante, "history is . . . the link in an uninterrupted chain. The literary composition closes its conclusion upon itself" [p. 172].)

Although Bann believes that history and fiction must be kept more or less distinct, fiction need not "have all the best stories" (p. 174); history, in its different way, may have some too. This reviewer is tempted to ask slyly whether rhetorical analysis must necessarily have all the best terms. The question is not trivial, for, as Foucault has taught us, how the terms of discourse are set is enormously important. Clearly, a measure of Bann's originality comes from his use of rhetorical terms and concepts. It is no less certain that his terminology will keep his contributions beyond the reach of most historians.

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DAVID C. LARGE and WILLIAM WEBER, editors.
Wagnerism in European Culture and Politics. Assisted

by ANNE DZAMBA SESSA. Ithaca: Cornell University Press. Pp. 361. Cloth \$34.50, paper \$14.95.

From the early 1850s on, Richard Wagner's compositions, opera texts, and prose writings inspired—or were drawn on to support—cultural and social criticism, reform programs, and innovation in the arts throughout the Western world. For decades Wagnerism constituted a vital cultural force, and this collection of essays on its nature and impact is to be welcomed even if not all contributors have fully solved the methodological difficulties inherent in their subject. Some of the most significant manifestations of Wagnerism occurred outside the societies and journals devoted to his ideas. This diffusion poses problems of definition and selection, the more so because Wagnerism blended with aesthetic, social, and political developments of very different origin. That the fount of Wagner's influence was his music is a further complication. Although William Weber correctly points out that the polemical writings Wagner published between 1849 and 1851 "made his name [known] all over Europe with a breadth and intensity that his music had not effected" (p. 45), had Wagner been merely a writer on musical theory and culture, his influence would not have spread as far and would have faded sooner. But music is ambiguous material for the historian of ideas, who may be tempted to ignore the aesthetic and technical complexities of a composer's work and instead concentrate on his theoretical and critical writings or on his relations with colleagues, patrons, and public. Not to address his nonverbal art is, however, to run serious interpretive risks.

The essays that confront this difficulty and seek to integrate Wagner's music in their treatment of Wagnerism form the stronger part of the volume. In an interdisciplinary tour de force Weber analyzes changes in musical taste in the early nineteenth century from a preference for contemporary music to a preference for dead, "classical" masters and notes a new insistence on the performer's faithfulness to the score and more attentive, "inwardly" behavior on the part of the audience. Wagner was a product of this "musical idealism," but he soon turned against some of its elements and reimposed the authority of contemporary—that is, his—music, while raising the ritual character of its performance to previously unknown heights. Weber's essay, which lays a rich historical foundation for the rest of the volume, ends by linking Wagnerism to the central place that new music occupies in today's musical world. Equally good is Marion S. Miller's study of Wagner's reception in Italy, which includes an excellent discussion of Wagner and Italian nationalism. Gerald D. Turbow and Bernice Glatzer Rosenthal offer illuminating accounts of the interac-

tion of Wagnerism with politics and cultural change in France and Russia.

The remaining two essays are less successful. Anne Dzamba Sessa considers British and American Wagnerians together, but the differences between the two cultures inhibit a clear line of argument. Confusion is compounded by the author's tendency to compress and to disregard chronology. An example is her discussion of the psychological effects of the music dramas, which in slightly over two pages proceeds from a truncated statement by "German emigre novelist Thomas Mann" to Shaw, Mark Twain, Victorian critics, novelists of the 1920s, William Butler Yeats, James Huneker, the English decadents, and others (pp. 270-72). David C. Large writes on "Wagner's Bayreuth Disciples," a subject that he hopes will also reveal something about German Wagnerism in general. This may have been an ill-advised strategy on two counts. The links between the small Bayreuth group and the broad stream of Wagnerism flowing through German society before and after the First World War are never firmly traced. On the other hand, the Bayreuth disciples, as Large points out, were mediocrities; concentrating on their alternately amusing and pathetic peculiarities means saying little or nothing about the more profound German analysts of Wagner and Wagnerism. The absence of Thomas Mann, save for one passing reference, is particularly striking, the more so since in a brief "Conclusion" the two editors single out Mann and Friedrich Nietzsche as "Germany's best-known commentators on Wagner" (p. 284). Nevertheless, the collection as a whole is a useful introduction to Wagnerism, and in parts much more.

PETER PARET
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GEORGE L. MOSSE. *Nationalism and Sexuality: Respectability and Abnormal Sexuality in Modern Europe*. New York: Howard Fertig. 1985. Pp. xii, 232. \$29.50.

The thesis of this suggestive little book can be briefly stated: bourgeois respectability and nationalism shaped attitudes toward sex, and these sexual attitudes, in turn, contributed powerfully to militant nationalism and the rise of fascism. To make his case, George L. Mosse draws on papers written for his seminars on the history of sexuality at the University of Wisconsin and Hebrew University. He uses the term "respectability" to indicate "decent and correct" behavior and a "proper" attitude toward sex; "proper" and "decent" are identified as "normal" and "natural." He reminds us that what is accepted as normal sexual behavior is a product of history, not of universal law.

The historical developments that determined ideas of sexual normality in modern Europe were, first of all, eighteenth-century Protestant religious revivals, most notably, German Pietism and English Evangelism. Both mounted formidable moral ramparts against the inroads of "unnatural" sex, endorsing the "normal" missionary position—that is, sex only for procreation. The two religious traditions, however, had different political consequences. German Pietism encouraged political quietism and obedience to *Obrigkeits*; English Evangelism encouraged active involvement in politics.

Nineteenth-century romanticism also influenced sexual attitudes by extolling the differences between heroically dominant males and delicately subservient females. The very idealization of women kept them in their place—and their place was either on an elevated pedestal or consigned to hearth and home. Walter Scott caught the spirit in *Ivanhoe* when he had Rebecca say, "God made women weak and trusted their defense to men's generosity."

In the bourgeois century the middle class apotheosized itself as the epitome of respectability and the guardian of sexual probity. This "moral majority" of another generation—which was, in Peter Gay's notable phrase, "publicly prudish and privately prurient"—insisted that civilization and national power rested on the inequality of the sexes and sexual propriety, both of which middle-class Europeans considered as natural and ineluctable as the economic division of labor they extolled. When bourgeois respectability was married to nationalism, one of the most potent alliances of the century was formed. It was symbolized by female figures: Germania, Britannia, and Marianne. As the century wore on, these chaste guardians of national virtue donned more clothing. Thus, Marianne, the bare-breasted activist who had held aloft the tattered flag of revolution as she womanned the barricades in Delacroix's famous painting of 1830, by the end of the century covered her chest with flowing robes and sat decorously as the quintessential symbol of stability and order. Germania became identified with Queen Luise of Prussia, a sort of secular saint and German madonna, the personification of domestic and moral virtues. Britannia was incarnated in Queen Victoria.

In all European countries patriotism was equated with sexual normality; "unnatural" sex, with national decline and racial corruption. By the end of the century, racism had joined respectability as a powerful ally of truculent nationalism. Together these three forces launched a sustained attack against all those "outsiders" who diverged from accepted norms of behavior. Particularly pernicious was the "accursed race" of homosexuals and Jews, as Marcel Proust (himself a homosexual of Jewish lineage) called them.

At the beginning of the twentieth century, Otto Weininger, author of the immensely influential *Sex and Character*—a book that greatly impressed Adolf Hitler—joined other racists to argue that a healthy Jew was a contradiction in terms and to urge the removal from society of all Jews and homosexuals. Both, he insisted, employed sex with diabolic cunning to undermine the nation's health through racial and moral pollution. The Nazis agreed. Donning the mantle of rectitude and declaring themselves the guardians of morality, they inveighed against the horrors of "Jewish depravity" and passed Draconian laws against homosexuality, pornography, masturbation, and other heinous practices that, their decrees intoned, were "offensive to the moral sensibilities of the German people." (The Nazis had no sense of irony.)

Mosse is right in saying that insufficient attention has been paid to the role that sexual respectability played in the intensification of nationalism and the rise of fascism. This unpretentious but useful book helps correct that deficiency.

ROBERT G. L. WAITE
Williams College

JOHN E. MARTIN. *Feudalism to Capitalism: Peasant and Landlord in English Agrarian Development*. (Studies in Historical Sociology.) Atlantic Highlands, N.J.: Humanities. 1983. Pp. xxii, 255. \$36.00.

"British academic historians do not like Marxism." Thus did Rodney Hilton sum up in 1976 his lifelong endeavor to promote a Marxist interpretation of history. He was not optimistic for the future since historians, having learned to throw out the Whig interpretation of history, have also learned not to espouse any other grand theory of history. For this reason John E. Martin has been bold, indeed, in attempting to combine sociology and serious history between the same hard covers. Employing the terms "feudalism" and "capitalism" in a Marxist sense, Martin sets out to provide a "reformulation of feudal economic and political structure and of the place of commodity relations so that the trajectory of feudal developments and the place of class struggle between peasant and landlord might be understood" (p. 214). The subtitle of his book, "English Agrarian Development," is hardly touched on.

The exposition alternates between chapters that deal exclusively with sociological theory and chapters that are primarily historical in content. In the chapters dealing with theory Martin first summarizes and provides a lucid critique of various Marxist theories of feudalism, absolutism, and capitalism; he then goes on to outline his own reformulation of the feudal mode of production. In the historical sections he provides a "detailed illustration of [his] ... re-

vised theory of feudalism by reference to medieval England" (p. xviii). He carries this over to the modern period, culminating with a summary of his own research on the 1607 antienclosure riots, a highly localized outbreak of discontent centered along the Jurassic ridge that forms the boundary between Warwickshire and Northamptonshire, a region characterized by late settlement and early desertion and thus an archetypal candidate for enclosure.

For the early modern section the author has read widely and wisely, but monocausal explanations are never safe. Rents and tenures are, of course, important, but the demographic and economic consequences of the Black Death and subsequent plagues were very far-reaching indeed and, like the sea-change in the educational, religious, and political life of England, cannot simply be ignored. Martin reconstructs the narrative of the 1607 riots but makes no attempt to analyze these events. One might ask, for example, were the malcontents genuine peasants or the lackeys of rival gentry? More important, what does Martin understand by the term "peasant" in the medieval and modern periods?

The medieval section, based on limited and often outdated secondary material, is the weakest part of the book. Michael Postan has simply been misunderstood, too many others—John Beckerman, Paul Harvey, Barbara Harvey, Michael Clanchey—have been overlooked, and the magisterial work that strikes at the heart of Martin's argument has received no mention at all (I refer to Paul Hyams's *Kings, Lords, and Peasants in Medieval England* [1980]). Martin's understanding of local custom is confused throughout, and he persists in equating it with estate rules.

The book is well designed and produced. I regret only the lack of a bibliography or, at least, of references in the index to the footnotes where such information can be found.

CICELY HOWELL
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JEFFREY G. WILLIAMSON. *Did British Capitalism Breed Inequality?* Boston: Allen and Unwin. 1985. Pp. ix, 270. \$28.50.

This study is an important contribution to a continuing debate that is frequently marked by polemic. But Jeffrey G. Williamson eschews polemic and brings together a wealth of statistical evidence relating to the issues involved. In addition, he constructs a macro-model of British industrialization and inequality to help identify the factors responsible for the pattern of change he finds. He also takes into

account the insights of nineteenth-century analysts of British capitalism.

Williamson finds that inequality grew from about 1760 to the 1860s, although this trend was checked somewhat during the wars with the French. From the 1860s to World War I, the direction of change reversed and inequality diminished somewhat. These results are seen to conform to the generalization made by Simon Kuznets concerning the progression of income inequality during a process of national economic development (*American Economic Review*, 45 [1955]: pp. 1-28).

Williamson relates the initial movement toward inequality to features of the Industrial Revolution in Britain. In particular, he stresses the unbalanced advance of productivity that favored sectors of industry making the most intensive use of machinery and labor skills. Yet supplies of skills were often quite inelastic. During the second half of the nineteenth century the rate of technical progress slowed, the advance of productivity became rather less uneven on a sectoral basis, and supplies of skilled labor became more elastic. These latter alterations encouraged greater equality.

The direction of change and the timing of its reversal, according to the author, do not appear to be associated with either demographic forces or international commodity price movements. Demographic factors, Williamson finds, "played no role in accounting for British inequality trends in the nineteenth century since it appears that the rate of conventional capital accumulation fully accommodated any quickening or retardation in labor force growth induced by previous demographic events" (p. 159). Price trends for internationally traded commodities seem to have dampened the growth of inequality during the early decades of the century. Subsequently, those trends ran counter to the factors that made for diminished inequality.

Economic historians with a direct research involvement in this area will wish to pay particular attention to the range of data that Williamson employs to reach his conclusions. He acknowledges that important gaps in the data remain to be filled (pp. 202-04). Of more general interest to economists and historians at large is the structure of the general equilibrium model presented in part 3 of the book. The model has strong roots in "classical" economic analysis. For example, capital and unskilled labor are assumed to move freely among all sectors of what is essentially an open economy. But the model also contains some "unorthodox" elements; most notably, it acknowledges the existence of noncompeting groups. Markets for skilled labor are segmented and, hence, class specific.

Early in the book the author states that he is aiming "to elevate and sharpen the debate over capitalism's success or failure" (p. 85). Whatever

specific criticisms they may wish to level, many readers are likely to find that Williamson has succeeded in fulfilling that aim.

BARRY GORDON

University of Newcastle

R. B. WERNHAM. *After the Armada: Elizabethan England and the Struggle for Western Europe, 1588-1595*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xxi, 613. \$64.00.

For four years after the Spanish Armada limped homeward in August 1588, England mounted a series of offensive operations in her war against Spain. They included numerous exploits aimed at harassing the enemy by sea. Yet, as R. B. Wernham argues, the Elizabethan government concentrated its efforts chiefly on the Continental land war, not only in the Netherlands but also, and especially, in northwestern France. Although certain of these efforts yielded positive results, most led to disappointment and frustration. Disillusioned, Elizabeth resumed a mainly defensive posture by the summer of 1592. The withdrawal of English troops from Brittany in February 1595 marked the end of a significant phase of military endeavor by "a small half-island of limited resources and manpower" (p. 563).

Skillfully constructed, colorfully and vigorously written, Wernham's book shows how the struggle "may have appeared to Queen Elizabeth and her advisers" (p. v), how many of their measures came to grief, and how disenchantment began to spread in Parliament and in the shires as well as in government circles. Although the queen poured over one million pounds and almost fifty thousand men into the war effort, her intelligence agents were unreliable, her forces under-equipped, her commanders self-willed, and her allies disinclined to dance to her tune. But Wernham has also hoped to show "in something like its proper perspective" (p. xi) that by 1595 "Elizabethan England had played a very considerable part" in preventing "Spain from establishing its control over the whole of western Europe, from acquiring the crown of France and destroying the Dutch republic" (p. 563). On these and related issues of interpretation the book is flawed.

It is flawed not, as some critics may complain, because the author relies heavily on one class of manuscript evidence: the foreign series of English State Papers, to which he has already devoted decades of editorial work. In the present state of knowledge no historian of sixteenth-century international affairs can reasonably be expected to acquire an equal mastery of relevant archival materials from all the countries with which he has to deal. Wernham has performed a major service in making available

to future scholars the fruits of so thorough an exploration of the English materials. The trouble is, rather, that his treatment remains throughout the book at the level of events. His narrative method allows too little scope for sustained analysis either of motives and personalities, even on the English side, or of the realities of European politics, even insofar as these are discernible from printed sources. In consequence, his observations on central figures and issues tend to be inconsistent or superficial: witness, for instance, his remarks on Elizabeth herself (pp. 266, 276), on the earl of Essex (p. 419), on Henry IV (p. 376), and on the implications of the structure of political authority in the United Provinces (pp. 31–33, 232, 485).

And inconsistency sometimes induces error, most seriously in respect to Henry IV's famous *Déclaration* of August 4, 1589 (N.S.). What the king carefully promised at that time (see p. 147) was, in fact, in line with conciliarist proposals that his Huguenot advisers had long been making on his behalf. But Wernham soon proceeds gratuitously to inflate it into a "promise to 'receive instruction' in the Catholic faith" (p. 188). This statement makes nonsense of France's continued agonizing over the question of the Bourbon's religion down to 1593.

Despite such flaws this book remains an impressive achievement, consummating its author's distinguished career and placing all students of Elizabethan foreign affairs more deeply than ever in his debt.

HOWELL A. LLOYD
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RALPH A. HOULBROOKE. *The English Family, 1450–1700*. (Themes in British Social History.) New York: Longman. 1984. Pp. vii, 272. \$14.95.

This study is a useful survey and critical analysis of current work on the early modern English family. Throughout, Ralph A. Houlbrooke displays the broad knowledge of English social history necessary to treat his subject accurately and sensitively. His mastery of the primary sources and secondary literature on the early modern family is also impressive, although he has missed a few articles that appeared in American historical journals and contain material he ought to have considered.

Houlbrooke's view of the early modern English family represents the emerging revisionist consensus that rejects the models of Lawrence Stone and Philippe Ariès. He argues that the nuclear family was the key family form from 1450 to 1700, as it is today, and that there have been no basic changes in ideas about the family, its functions, or emotional relationships among its members. His emphasis is on continuity rather than change. He minimizes Protestantism as a force for change in the family,

although he does note that Protestants may have emphasized the family's religious functions more than Catholics did. What is perhaps most helpful is that he brings the writings of humanists as well as those of Catholic and Protestant publicists and clergy into his discussions of sixteenth-century ideas on the subject.

Like Linda Pollock in *Forgotten Children* (1983) and Keith Wrightson in *English Society, 1580–1680* (1982), Houlbrooke rejects the negative view of parent-child relations advanced by Stone, Ariès, and many of the authors included in Lloyd deMause's *The History of Childhood* (1974). What emerges is a much more convincing picture of parents who loved their children, made considerable emotional and financial investments in them, and mourned deeply when they died. The structure of early modern society was such that parents expressed their concern and love differently from late twentieth-century parents: they often placed their children in other people's households at ages that seem unacceptably young to us or insisted on the right to arrange their offsprings' marriages against the latter's wishes. These strategies were adopted not because parents were indifferent to their children but because parents believed that they would secure their children's future happiness and prosperity. To be sure, some parents and children disliked each other or could not get along—some parents even abused their offspring—but the more evidence we accumulate, the clearer it becomes that negative examples should not be accepted as typical just because they support Stone's and Ariès's conceptual models.

Houlbrooke's interpretation is convincing because he supports it by a careful consideration of the quantity and character of the available evidence and because he draws heavily on primary sources. In addition to its other merits, this book is clearly and concisely written. I recommend it to historians, students, and the wider public as a first-rate introduction to the early modern English family.

BARBARA J. HARRIS
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ANTONIA FRASER. *The Weaker Vessel*. New York: Alfred A. Knopf. 1984. Pp. xvi, 544. \$19.95.

Antonia Fraser's *Weaker Vessel* provides a fascinating and comprehensive portrait of women in seventeenth-century England. Her book encompasses all aspects of women's lives: educational, sexual, marital, and maternal, as well as their involvement in the religious, social, and political movements of the 1600s. The work's strength lies in Fraser's portrayal of the variety and fascination of individual women's lives. Its weakness, in many ways, emerges from these strengths.

Fraser employs an expansive canvas on which to re-create the choices, directions, and warnings confronting women during the 1600s. She divides her material into three distinct periods: part 1, "As It Was—This Blessed Knot," takes the story approximately to 1640; part 2, "With the War—Stronger Grown," to 1660; and part 3, "Afterwards—A Continual Labor," to the end of the century. Although one can discover some change in women's lives during these periods, similarities heavily outweigh differences, and some of the changes Fraser emphasizes are unconvincing. Certainly, women married during all three periods, and little changed in the legal or economic realities of marriage. For example, Fraser argues that in mid-century parents allowed daughters greater freedom in selecting a mate, but she then includes numerous examples revealing that during the post-1660 period many parents controlled their daughters' marriages as strictly as during the early years of the century.

Although it is justifiable to stress women's public valor during the Civil War, the few heroines and their less prominent sisters who defended castles and aided soldiers do not constitute a changed status for women as the author suggests. A widow taking over her husband's printing business in the 1630s exemplifies the same qualities of independence and leadership as a mistress defending her home during the Civil War. Finally, it is unclear why Fraser focuses on the grind of women's daily work for the period 1660–1700. Women obviously labored throughout, and there is no evidence that work became increasingly onerous as the century progressed. In sum, the author claims patterns of change for phenomena showing little or no variation during the seventeenth century.

The best portions of this book are found in those chapters that are naturally limited in the scope of their subject. One such chapter, "A Solicitating Temper," is devoted to women's efforts to regain lands that had been sequestered under the Commonwealth because of their owner's royalist sympathies. "Divorce from Bed and Board" discusses the varieties and alterations in marital separations during the late seventeenth century. In these chapters the author outlines a subject thoroughly and limits her examples to a manageable number. This allows her to go into sufficient detail about the lives of each individual in order to build a portrait that gives space to motivation and change as well as description.

Unfortunately, most chapters include lengthy and sometimes rambling accounts of less limited subjects such as education, religious zeal, courtship, marriage, and motherhood. In these chapters the author presents a general introduction to the topic and concludes with numerous examples, normally extending to a page or less. Here is a wealth of

information, but it is wealth often lost because of its volume and repetition. Also, throughout the work Fraser refers back to women introduced one hundred or more pages before and, with only a brief reminder, expects the reader to remember the subject. That is often expecting too much. Unless one has a long-term familiarity with the leading families of England and their permutations during the 1600s, one is left drowning in a sea of examples.

At times, Fraser's examples become especially random. For instance, her chapter "Living under Obedience" details the nature of seventeenth-century marriage and considers women's education, their economic options as maids, their minor role as highwaypersons, and Lady Eleanor Davies's (Douglas) religious mysticism. It is difficult to understand why one would include such a strange assortment of characters and events to understand something as ubiquitous as the demand for obedience in women.

Some changes that did occur are lost in Fraser's structure. There were advances for women, especially regarding literacy and publications, near the end of the century. But Fraser fails to tie these changes to her progression because she contends that women returned to more traditional roles following the Restoration. Generally, there was little significant change for women during the seventeenth century. Whether one emphasizes the static nature of women's lives in the seventeenth century or concentrates on the few areas of improvement, neither conforms to the contours of *The Weaker Vessel*.

The most serious flaw in Fraser's work is its lack of a theoretical framework to which to connect her myriad examples. A related problem involves her omission of recent scholarship that might have provided her with an explanatory model. Although the work possesses a substantial bibliography, it is almost devoid of article literature about seventeenth-century women. Fraser tells much about the lives of numerous seventeenth-century women and includes some materials about women not from the upper classes. But one leaves her work with a series of half-remembered figures, little clear understanding of the general nature of women's lives, and few useful guides for analyzing how and why women's lives developed as they did and whether, indeed, they experienced significant change during the 1600s.

HILDA L. SMITH

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GALE E. CHRISTIANSON. *In the Presence of the Creator: Isaac Newton and His Times*. New York: Free. Pp. xv, 623. \$27.50.

This most recent biography of Isaac Newton, intended for the general reader and, we may presume, students, relies on psychological theories to provide explanations where historical evidence eludes us. In Gale E. Christianson's hands the subtlety found in Frank Manuel's fascinating psychological portrait of Newton disappears. We are left with a tortured genius in possession of "memories of a childhood spent in a chronic state of emotional dislocation because of a mother who absconded with a stranger." Newton's widowed mother had married again and did not bring her son into the new household. This deeply scarred young man "could no more forge a bond of deep affection with his Creator than his fellow man." In later life, it should be noted, this same man wrote ecstatically of the future millennial paradise wherein the saints would reign triumphantly with their creator. Newton related, we are told, to his chamber mate at Trinity College with "smothering possessiveness." In fact, practically nothing is known about the emotional side of their relationship, which always remained cordial. At Newton's first meeting with Robert Boyle we are told "that melancholy countenance came face to face with melancholy countenance." Out of this dreary setting came some rather important history.

Despite this heavy psychologizing, Christianson has attempted to master that history; indeed, the opening chapters on Newton's historical context are well done. He tends, however, to see all and sundry as Puritans without coming to terms with the Anglican associations of Newtonian natural philosophy or with the obviously royalist or Anglican proclivities of scientists like Isaac Barrow or philosophers like Henry More. The technical exposition of Newton's science is helpful and clear; the discussion of alchemy, heavily indebted to B. J. Dobbs and R. Westfall, makes a good read.

The greatest difficulty with the book, given its audience, is its sheer size and wordiness. A heavy editorial hand should have been applied before publication. Books like this one come to exist less because of their subject—fascinating though it is—than because of the growing specialization within the history of science. If we are to continue to teach the history of science within general history courses, texts of brevity, yet also of clarity and sophistication, will be needed, and in that regard this book does not achieve its aims.

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The essays in this volume address a subject that the work of Christopher Hill has made central to the historiography of seventeenth-century England. Indeed, if Hill's *The World Turned Upside Down* were not so good, I might use a paperback edition of these essays to introduce my students to the historiography that describes the sectarian religions during the revolutionary decades.

I cannot, of course, summarize the contents of a collection of essays, but a few comments may indicate that competent authors have looked at various aspects of an interesting cultural theme. J. F. McGregor notes the difficulties in classifying sub-species of sectarians like the Ranters and the Seekers. Brian Manning insists once again on the class nature and interrelationship of religious and political conflict. B. Reay provides a good survey of recent writing on Quakerism and remarks on the incoherence and perverse political results of Quaker ideology. Bernard Capp gives us a predictably solid piece on millenarianism. And the printing of Hill's Barnett Shine Foundation Lecture for 1974 on the theme of irreligion in modern revolutions sets one gem in the collection. The only real disappointment is G. E. Aylmer's piece, "The Religion of Gerrard Winstanley," which invites unflattering comparison with Hill's brilliant essay published as a *Past and Present Supplement* in 1978.

From my perspective there are some basic problems with the editorial organization as well as with the conceptualization of this presentation of revolutionary religion. Why, I wonder, is there no essay on the fascinating and neglected topic of the law reforms proposed by religious radicals? I also wish some attention had been given to the even more neglected subject of the aesthetic aspects of religious radicalism—to the radicals as writers and artists. More particularly, in terms of what is done the editors strike me as theoretically leftish jejune. In the preface and introduction Reay and McGregor invoke the almost meaningless jargon of the sociology of "ordinary people" while declaring religious radicalism a political failure. The latter is a very myopic view. Undergirding all is a characteristic naïveté about Puritanism as a definable entity and a mistaken belief that Puritans were radicals.

The real justification for these essays, however, is that they do indeed represent expert writing on an interesting slice of English revolutionary experience that has found at least a temporary consensus among its historians.

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J. F. MCGREGOR and B. REAY, editors. *Radical Religion in the English Revolution*. New York: Oxford University Press. 1984. Pp. ix, 219. \$32.50.

VINCENT J. LIESENFELD. *The Licensing Act of 1737*. Madison: University of Wisconsin Press. 1984. Pp. xiv, 259. \$27.50.

Although many would disagree with its being called a "central historical event" (p. 3), the Licensing Act of 1737 is of interest to students of politics in the era of Robert Walpole and of the English theater as well.

Newspapers, pamphlets, and cartoons in eighteenth-century England were free to the point of license in their treatment of public figures, including royalty. The stage was an exception: from 1737 under the provisions of the Licensing Act, all new theatrical productions had to be approved in advance by the lord chamberlain, who could forbid the performance if he found it for any reason to be not "fit." There was no provision for appeal.

The usual explanation for this act is that, outraged by satirical pieces by John Gay, Henry Fielding, and others, Walpole and King George II pushed the censorship through Parliament. Vincent J. Liesenfeld agrees that sensitivity at court was part of the story but is at pains to show that other factors were involved as well. For example, the act reaffirmed the monopoly rights of the Drury Lane and Covent Garden theaters, which held royal patents. This pleased the patent owners and their friends, though not the actors, whose employment opportunities were curtailed by the forbidding of independent theaters. More important, this provision pleased many respectable London tradesmen and employers, who feared new theaters as a type of urban blight, attracting prostitutes and sharpers to the neighborhood and tempting apprentices to idleness or depravity. Thus, moves to curb the theater had the influential support, for example, of Sir John Barnard who, though an opponent of Walpole, was also strongly religious and a spokesman for the lesser merchants in the City of London.

Liesenfeld's book is essentially a legislative history of the act of 1737 and its aborted precursor of 1735; the book simply stops with the act's passage and says nothing about its short- or long-run effects on the English theater. There are slips: Restoration Day is May 29, not May 30 (p. 136); the peer mentioned on page 149 is most likely the second Earl Stanhope, not his kinsman Chesterfield. There is rather too much detail about the law and about parliamentary procedure, which occasionally makes for heavy going. But the book is sensible, well written, and thoroughly researched and documented. It will fill a narrow but real niche in eighteenth-century studies.

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NORMAN GASH. *Lord Liverpool: The Life and Political Career of Robert Banks Jenkinson, Second Earl of Liverpool, 1770-1828*. Cambridge: Harvard University Press. 1984. Pp. xvii, 265. \$20.00.

Robert Banks Jenkinson, Lord Hawkesbury and second earl of Liverpool, served as British prime minister from 1812 to 1827, overseeing the fall of the French empire, the containment of domestic radicalism, and the liberalization of commercial policy. Along with Gladstone and Lord Wilson of Rievaulx, he won as many general elections as any prime minister in British history. Yet in popular perception Liverpool lacks a prominent position in the prime ministerial pantheon; far below an acknowledged "great" such as Walpole or Lloyd George, he hovers someplace above Goderich or Bonar Law but well below Salisbury or Grey. Many of Liverpool's contemporaries thought him a second-rater too: he was poor "Jinksbury" to Canning, the "Dog Liverpool" to Byron, and the "Arch Mediocrity" to Disraeli. For well over a decade Norman Gash, J. E. Cookson, and other scholars have attempted a Liverpoolian rehabilitation. By now they deserve to have succeeded, although it is doubtful that the general reading public will ever warm to Liverpool.

Son of the most pronounced "king's friend" of the age, son-in-law of a Voltairian bishop, and brother-in-law of one of the participants in a famous ducal *ménage à trois* at Devonshire House, Liverpool, surprisingly, turned out a very model of constitutional propriety and Christian morality. Such attributes, in Gash's view, help explain the respect shown him in the country at large, whatever the attitude of smart London society. Yet Liverpool was not only a spiritual father of the Victorians. In his most compelling thesis Gash assesses Liverpool as "one of the great though unacknowledged architects of the liberal, free trade Victorian state" (p. 253). Indeed, it is not Liverpool who tended toward mediocrity but Grey and Melbourne's Whig administrations of 1830 to 1841, which served as a "mere interlude" in the "continuity of economic policy" (p. 253) between Liverpool and Peel and then beyond to Gladstone.

This volume makes no pretense to being the definitive modern life of the prime minister. Of unpublished manuscripts, Gash only really used the massive Liverpool collection in the British Library. Nonetheless, he has written a splendid short biography that, if it yields to W. R. Brock's *Liverpool* (1941) in stylistic elegance and to C. D. Yonge's *Life* (1868) in bulk, is a model of its genre.

A few odd mistakes and solecisms creep in here and there. Augustus Frederick, duke of Sussex, was not third in line to the throne after the death of Princess Charlotte (p. 133). Sussex's paternal grandmother, Augusta of Saxe-Gotha, was not queen dowager of Great Britain (p. 9), though, probably, like the duchess of Kent she would have enjoyed the title. When Pitt resigned in 1801, he had not held "a longer period of unbroken power than any of his predecessors" (p. 37). The twenty-one prime minis-

ters whom Gash counts between 1714 and 1830 might more usefully be listed from 1722. And, surely, Walpole and North would have been shocked to know that Liverpool encountered a "comparatively recent innovation in public life, the presence of an organised parliamentary opposition" (p. 4). Finally, Gash calls Lady Holland "one of the most scandalous ladies of her generation" and as notorious as Queen Caroline (p. 32). Is this not a bit hard on Old Madagascar?

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PIERS MACKESY. *War without Victory: The Downfall of Pitt, 1799–1802*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xi, 248. \$45.00.

How was Britain to conduct the war against France following the failure of an Anglo-Russian expedition to Holland and the subsequent evacuation of forces in October 1799? Having already analyzed that expedition in detail in *Statesmen at War: The Strategy of Overthrow, 1798–1799* (1974), Piers Mackesy turns again to the king's political leaders for an answer. Drawing on standard manuscript and printed sources, he reconstructs, mainly for 1799 and 1800, the way William Pitt and two others gave direction to the war effort.

The government, as Mackesy persistently demonstrates, could not maintain a successful or consistent strategy. Unwilling to exercise leadership as a prime minister, Pitt moderated between the opposing views of George Grenville, then foreign secretary, and Henry Dundas, the secretary of state for war. The former pushed for engagements on the Continent to overthrow the revolutionary government; the latter recommended a maritime strategy and attacks on French colonies.

External factors confounded the king's ministers. Napoleon forced the British to respond to his aggressions. Allied efforts with the Russians and Austrians proved difficult in negotiation and execution. Hamstrung by divided councils and fettered by the nation's reluctance to raise a large army, the ministers grasped at expedients, just as the elder Pitt had in the dark days of 1757 and 1758. Chapter 3, "Plans and Illusions," shows how concentric attacks on France floundered on false hopes, not the least being the usefulness of the Russian soldiers, previously evacuated from Holland to Great Yarmouth, who impressed one lady of quality as "half-starved savages" (p. 53). Ministerial attention turned to an amphibious landing on the French coast. That idea had been tried frequently in earlier wars against the France of Louis XV and had experienced the greatest tactical success in the 1761 capture of Belle Isle.

Mackesy's narrative demonstrates the sad truth that those raids taught lessons that had been forgotten. In preparing for a new assault on Belle Isle in 1800, the fighting services lacked accurate charts, sufficient transports, trained soldiers, and commanders willing to take chances. Pitt needed, but did not have, what his father found in James Wolfe years earlier at Quebec—a commander willing to take calculated risks.

In chapter 9, "The Fall of the Ministry," Mackesy sees the Irish Catholic question as the problem that finally broke Pitt's will to continue and led to his resignation in February 1801. Mackesy's sixteen pages contain suggestive arguments that John Ehrmann should address in the forthcoming third volume of his authoritative study of Pitt.

War without Victory is something of an afterpiece in comparison to Mackesy's three other works on Britain's wartime difficulties from 1775 to 1810. Although less profound in scope and treatment than his *The War for America, 1775–1783* (1964), the monograph under review contains valuable insights concerning the muddle of planning and the ominous implications for any power—great or small—that is unable to rise boldly to new challenges.

WM. KENT HACKMAN
University of Idaho

DAVID HEMPTON. *Methodism and Politics in British Society, 1750–1850*. Stanford: Stanford University Press. 1984. Pp. 276. \$27.50.

According to the much-discussed thesis of Elie Halévy, Methodism was a central stabilizing force in industrializing Britain. In this view those working people who joined the Methodist societies became frugal, diligent, loyal to government, and contented with their lot. The reality, David Hempton shows in his rewarding book, was very different. There was no simple commitment to the Protestant work ethic, least of all by John Wesley himself, and Methodists were in any case too few (4.5 percent of the population at their peak) to inhibit radicalism in society at large. Furthermore, concentration on the Halévy thesis has thrown into the shadows many other significant aspects of the relationship between Methodism and politics up to the mid-nineteenth century. Hempton brings these issues back into the light. Although he is prepared to disagree on points of detail, he broadly concurs with the pattern of interpretation set out by W. R. Ward in *Religion and Society in England, 1790–1850* (1972). By 1830, Hempton argues, Methodism had largely lost touch with the common people because its officials suppressed radicalism and revivalism. There was a steady drift toward a social conservatism expressed through more-or-less overt allegiance to the Tories.

The process was begun by Wesley's own conservative tendencies, although Hempton sees these not as part of a systematic philosophy but as responses to particular situations. After Wesley's day the leaders were eager to discourage radicals like Alexander Kilham (a disruptive figure made intelligible for the first time by this book) and even more eager to demonstrate to the government, for fear of restrictions on gospel preaching, that they were doing the discouraging. A fierce anti-Catholicism, fostered by rivalry with Roman Catholics in Ireland, pushed them further toward Toryism, as for a while did hopes that a Tory administration might endow Wesleyan schools. The result was Jabez Bunting's attempt to impose a tight discipline, which prevented more liberal voices in the denomination from being heard. Yet liberal voices there were, since there was an underlying trend for the rank and file to take an entirely different political course. In the 1830s, except in unusual circumstances, Methodist voters were Whig/Liberals. Although the relationship was complicated, Methodism could even foster radicalism.

With apt phrases and a lively style, the author frequently reminds us of the ambiguities of his subject. More could perhaps have been made of the providentialism of the Methodist world view, the repercussions of missionary rivalries beyond Ireland, and the antislavery crusade. But this book successfully blends original research with a survey of the scattered secondary literature to produce a study that will be widely used and appreciated.

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JULIA STEWART WERNER. *The Primitive Methodist Connexion: Its Background and Early History*. Madison: University of Wisconsin Press. 1984. Pp. xv, 251. \$35.00.

This excellent volume takes as its purview the background and first decade of English Primitive Methodism, an 1811 offshoot of the original Methodist connection in Britain. This study by Julia Stewart Werner is the first major effort in this field since 1906. Heretofore, most work on the group was written by the nineteenth-century Primitive Methodist clergy, whose studies often bordered on hagiography. The difference of viewpoint in Werner's study is well indicated by a single instance. Writing in 1820, Hugh Bourne, the cofounder, saw the revival in the East Midlands as "providential." Werner argues that "in fact, the rapid progress . . . occurred because a complex of social and economic factors had created a demand among the poor for exactly what the connexion was able to offer" (p.

135). Her volume seeks to describe these factors *in extenso*.

Scholars of American church history will find this volume almost as interesting as English historians will. The "Prims," or "Ranters," adopted a highly emotional evangelistic rural ministry, centering on the introduction of camp meeting techniques, in the same decades (1800–20) that a similar ministry was finding great success on the American frontier. In the person of Lorenzo Dow the two movements even found a common leader. Although not Werner's primary concern, the similarities of the movements on either side of the Atlantic are striking. The dissimilarities are equally striking. Mainline American Methodism did not prove so brittle as to shatter under the impact of the camp meeting and democratically oriented leaders like James O'Kelley, whereas similar issues raised by the "Prims" in Britain produced major fractures in the body ecclesiastic.

Inevitably, the Halévy thesis raises its head in such a volume as this. Werner is advisedly guarded in her speculation about the relation between political radicalism and the "Prims'" success. She finds inadequate the oft-repeated assertion that the "Ranters" were little more than political radicals under an assumed guise of religious enthusiasts. Her careful array of "conclusions" and "conjectures" on this much-mooted question (pp. 173–76) is worth careful study by political as well as church historians.

Although the study gives us a good introduction to Primitive Methodism, one fails to get much insight into what made the typical "Prim" tick. In the history of a movement so mighty in its effect on individuals, one would wish for more personal witness and data. This is probably more a problem of the sources than a criticism of Werner. Most of the "Ranters" were not of the class who could read and write, and there are few letters and journals giving private entry into inner thoughts or religious feelings. Much has to be inferred from the writings of others, some of whom were hostile to "Prim" piety. Werner has probably done the best that can be done with the sources available, but one cannot but mourn that the individuals who move across these meticulously documented pages are not more personally alive and vital for the reader.

EARL KENT BROWN
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B. R. MITCHELL. *Economic Development of the British Coal Industry, 1800–1914*. New York: Cambridge University Press. 1984. Pp. xv, 381. \$59.50.

MICHAEL W. FLINN. *The History of the British Coal Industry*. Volume 2, 1700–1830: *The Industrial Revolution*. Assisted by DAVID STOKER. New York:

Clarendon Press of Oxford University Press. 1984. Pp. xviii, 491. \$67.00.

Even the most committed student of British coal mining will approach two more detailed histories of this great industry with some trepidation. Such anxiety is not initially allayed by B. R. Mitchell's volume, for it reveals something of its origin as a Ph.D. thesis started more than thirty years ago. The style is somewhat labored and the approach resolutely economic. "It is ... not a comprehensive history of the coal industry in the period, for aspects of its social history are touched on only as they impinge on its economics, and attention to the actual selling of coal is only peripheral" (p. xi). The book is based on the meticulous reworking and interpretation of the evidence provided by such well-used sources as parliamentary select committees, royal commissions, and the reports of mine inspectors. This approach yields both detailed statistical data and a number of persuasive arguments. Mitchell claims, for example, that throughout the nineteenth century the majority of capital invested in coal mining was provided by the industry's owners and managers and that by the second half of the century the expansion of production was determined largely by a growth of demand "that was, for practical purposes, independent of price levels" (p. 34). He provides both an illuminating account of the employers' failure to introduce more coal-cutting machinery in the decades before the First World War and a careful discussion of labor recruitment, arguing "that the generally accepted view that the mining community itself was by some way the most important generator of new recruits holds good prior to the 1840s and after 1874; but in the interval, flows from outside were of roughly equal importance" (p. 120). These are important issues, and Mitchell's painstaking reconsideration of the available evidence places in his debt all historians of the nineteenth-century coal-mining industry.

If Mitchell's book is one to respect, Michael W. Flinn's is one to savor. Any reservations about sponsored history—for this forms part of the National Coal Board's ambitious five-volume history of the industry from the Middle Ages to the present day—are dispelled immediately. Although Flinn's history is based on a prodigious amount of archival research in libraries and record offices across the British Isles, this great mass of material never threatens to become overpowering. Indeed, one of the most valuable aspects of this beautifully written study is that it examines the history not just of the industry but also of the entire coal trade: capital requirements, ownership, management, and profits; transport and marketing; mining communities and the conduct of industrial relations; and, in a brief concluding chapter, the relationship between the coal industry and

the economy as a whole. Flinn is at his best when dealing with technological and economic developments. He shows, for instance, that between 1700 and 1830 the real capital cost of mining fell while the real cost of coal remained remarkably stable; he concludes that, although profits fluctuated fairly sharply in the short- to medium-term, "collieries in the eighteenth and early nineteenth centuries generally made a profit approaching thirty per cent of their costs" (p. 323). These and almost all the other issues discussed in this outstanding book are dealt with scrupulously, elegantly, and convincingly. Flinn died suddenly before the publication of this, his final work. As other reviewers have pointed out, this exceptional book stands as a fitting memorial to a fine historian.

That Mitchell's volume should suffer when set alongside Flinn's is scarcely surprising. So surely would almost any book. Yet thanks to both of these new studies—and Flinn's in particular—a great deal more is now known about the British coal industry in the two centuries of unparalleled growth between 1700 and the outbreak of the First World War.

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ANTHONY HOWE. *The Cotton Masters, 1830–1860*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1984. Pp. xii, 359. \$39.95.

Every student of European economic history learns that the Industrial Revolution began in Britain with the dramatic transformation of the organization of production in the cotton industry. Similarly, every course in European political history recognizes the role of the Industrial Revolution in the generation of a new manufacturing class, first in Britain, then in a sequence of other countries. But, despite the attention that has been lavished on these twin themes, no historical study has united the economic characteristics of the cotton masters with their political aspirations and achievements. This well-researched work endeavors to bridge the gap between the two scholarly traditions.

Anthony Howe's principal technique is to analyze the careers of 351 Lancashire cotton masters on whom there is adequate information. He does not pretend that his selection is a representative sample of the four thousand or more textile entrepreneurs in Lancashire at the period but believes that this is the best that can be done. The group no doubt includes all the significant political figures but may give a misleading impression of the economic structure of the class. The chosen period is intended to bring the second generation of successful manufacturers into sharper focus, but in practice it proved difficult to confine the analysis to the period, and

Howe ranges from 1800 to 1870. Readers will appreciate the breadth of coverage but may still be disappointed that the author did not break out of the bounds of his D. Phil. thesis to offer a chapter or so on the years in which the explosion of energy in the cotton industry coincided with the ferment of ideas generated by the French Revolution.

The second generation of textile entrepreneurs is represented as a predominantly hereditary nucleus around which lesser (and anonymous) men collected to follow their economic and political leadership. Proud of their success, the elite seldom deserted their urban environment for landownership, preferring to invest their wealth in railways, banks, shipping, steel, and other growth points of the economy. After the passage of the Municipal Corporations Act (1835) they dominated local government and the social life of the Lancashire towns, patronizing philanthropy, culture, and education and winning social acceptance. Following the Reform Act, however, divisions between Whigs (Non-conformists) and Tories (Churchmen) were exposed at Westminster, and it seems that the majority influence was only crucial in the Anti-Corn Law League. Nevertheless, Howe believes they were less easily assimilated into Victorian society than were other commercial and industrial elites of the period. The contrast drawn with the City of London is too superficial, and we must wait for other studies of this quality before we can be sure that the contrast with Birmingham, the Potteries, and the Brewers is justified.

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IVOR WILKS. *South Wales and the Rising of 1839: Class Struggle as Armed Struggle*. (The Working Class in European History.) Urbana: University of Illinois Press. 1984. Pp. 270. \$31.50.

Inspired by a combination of Marxism and Welsh nationalism, Africanist Ivor Wilks has written on an episode of Chartist history. He states his thesis emphatically: what happened at Newport was not a riotous phase of the Chartist agitation for parliamentary reform but rather a manifestation of the "new proletarian class consciousness" containing a "subsumed historic sense of nationality" (p. 235). Class struggle took the "advanced form of armed struggle" (p. 249) as workers strove for a "workers' commonwealth" (p. 184). What made these developments go so far so fast in South Wales was that the capitalists and managers were English and the exploited were Welsh.

Historians usually write about the Newport rising as a confused set of episodes in a grim part of Wales long noted for violence. Fewer than two dozen

people were killed, and close to one hundred pounds in property was destroyed. Therefore, the fundamental question is whether the author has made too much out of the event.

The book does have weaknesses. It is alarming to see A. R. Schoyen's study from 1958 and David Williams's study from 1939 relied on so heavily while over a quarter of a century of the most recent work on Chartism is ignored, except for the local studies on South Wales. Incorporation of more current research on Chartism might have helped Wilks deal with the vast outpouring of romantic rhetoric glorifying physical force. Use of works on romanticism and revolution in early nineteenth-century Europe would have added perspective.

Uncovering the plans for an uprising that failed is not an easy task. The sources rest in a murky world of rumor and speculation, often poured forth by deluded, desperate, or deranged men on trial for treason. Wilks admits that much of his evidence is "refractory . . . hearsay . . . partisan and recriminatory"; some of it he found to be "pure fabrication" (p. 162). Yet the method he adopts to handle ambiguous sources is to present masses of often stupefying detail.

The book is strongest in the extensive sections that focus on South Wales rather than on Chartism. The author provides highly detailed information on the social and economic background of the region.

In all probability the true significance of the Newport rising will always remain the subject of controversy. It is nevertheless remarkable that so many Britons take pains to celebrate every violent episode in their modern history, which is so noted for lacking revolutionary violence.

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IAN ANSTRUTHER. *Oscar Browning: A Biography*. London: John Murray; distributed by Merrimack, Salem, N.H. 1983. Pp. x, 209. \$18.00.

If Oscar Browning was not the most famous Cambridge don of the late nineteenth century, he was probably the most famous eccentric one. He was the subject of numerous drawings in *The Granta* (many of which are reproduced in this biography) and of equally numerous stories illustrating his personal flamboyance and snobbish love for aristocratic youths and royalty of all ages.

Ian Anstruther's brisk biography sheds considerable light on the most critical event of Browning's life: his ejection from an Eton housemastership after fifteen years of service. Although the dismissal was officially attributed to Browning's failure to obtain the headmaster's formal permission to have more than forty boys in his house, the true cause was Browning's unwillingness to control his "crush" on

George Nathaniel Curzon, then a handsome and aristocratic schoolboy in another house. When protests against the headmaster's decision proved unavailing, Browning had to fall back on his life fellowship at King's College. It is ironic that, but for this untoward circumstance, Browning probably would never have returned to Cambridge.

Anstruther notes that Browning was extraordinarily lucky to have avoided public scandal. The Eton debacle did not induce him to alter his behavior, and, in a period of heightened sensitivity to homosexuality owing to the Oscar Wilde case, it was little short of miraculous that Browning managed to avoid a similar fate, especially considering that his meticulously preserved correspondence contained much incriminating evidence and many threats of blackmail. At Cambridge he became increasingly well known as a "patron of youth" with a weakness for handsome boys and a willingness to dip into his pocket to help them. There was clearly a strong homoerotic element in these relationships, although the precise degree to which they included sexual acts remains obscure.

Anstruther generally shows good judgment in his discussion of the intricacies of Browning's sex life, although occasionally his speculations become confused; for example, at one point he concludes that Browning was probably an active sodomite yet at another point suspects that Browning may have been impotent. This reader, however, would have liked to know more about Browning as a teacher of history. Although he was an inaccurate (though prolific) historian, Browning was clearly a gifted and charismatic teacher of the subject. It seems that he was largely responsible for introducing the teaching of history into classics-dominated Eton and, as director of studies in history at King's, he appears to have been instrumental in establishing that college's record of success in the history tripos. Browning was also notably successful as the first principal of the Cambridge Day Training College, a scheme that brought young men, generally of humble origins, to Cambridge for a combination of teacher training and the usual degree course.

Anstruther is not an expert in the history of education, and he occasionally serves up an old chestnut—such as Thomas Arnold's crucial role in the reform of the public schools. In general, however, he provides a lively account of an extraordinary character: a man of great energy and personal gifts whose life and career were marred by his sexual extravagances.

ARTHUR ENGEL

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DAVID DILKS. *Neville Chamberlain. Volume 1, Pioneering and Reform, 1869–1929*. New York: Cambridge University Press. 1984. Pp. xv, 645. \$29.95.

Here is the latest, fullest, and best-balanced account of Neville Chamberlain's life and career up to 1929. It has been almost forty years since the authorized biography by Sir Keith Feiling appeared, and many new source materials are now available. Along with the official records of Chamberlain's political life and the official and personal papers of many contemporaries, David Dilks has made exhaustive use of Chamberlain's personal archive, including his weekly letters to his sisters, in which he revealed his thoughts and feelings with considerable candor. The reader thus learns at nearly every turn what Chamberlain was thinking and why. But, as Dilks is aware, this is also a problem, for some fairly crucial matters rely for their interpretation largely on Chamberlain's account.

The work is divided into three equal parts. The first chronicles Chamberlain's family background, youth, six-year adventure on Andros Island in the Bahamas, and entrance into the business world of Birmingham and, later on, city government. His career from the time he entered national politics (at age fifty) through the first Baldwin government, in which he served briefly as minister of health before moving to the Exchequer, comprises the second segment; the third deals with the period 1924–29, when, as minister of health, he compiled an enviable record of accomplishment and became involved in the broader affairs of government and the Conservative party.

Commendably, Dilks does not approach Chamberlain's life and career as if everything pointed to his ultimate experience as prime minister (1937–40) and to the role in foreign affairs for which he will always be best remembered. While pausing at times to describe personal characteristics or qualities of mind and behavior that help in understanding the nature of the man, he treats each stage of Chamberlain's career as an end in itself, just as Chamberlain saw it. Inclining more toward his subject's life than his times and emphasizing, as good biography does, the point at which the two interacted, the author nevertheless elaborates, in the last section of the book, the intricacies of British party politics to a degree that may weary some readers. This provides, of course, the sharpest possible picture of the milieu in which Chamberlain labored.

Although Dilks concedes that no Chamberlain biography could fail to be controversial, the picture of the man that emerges here is neither out of line nor strikingly revisionist. Destined for a career in business by his famous father's decision, Chamberlain obediently accepted his lot and worked diligently to fulfill it. He demonstrated business acumen, developed an early interest in social reform, and became deeply involved in the civic affairs of Birmingham. It was not by dint of personality, showmanship, or charisma that he got ahead but by

hard work, efficiency, reasoning power, and the ability to make decisions. Unambitious politically, he was driven to bigger things by an inherited sense of duty, impatience with the inadequacies of others, and the sycophantic urging of his wife and sisters. His move into national politics was related to the exigencies of the world war, and he owed his quick advancement in part to the absence of able senior figures in the postwar Conservative party. Both Andrew Bonar Law and Stanley Baldwin, but less his half-brother Austen, found him to be a reliable political counselor. The Ministry of Health suited his special interests in local government, and the contribution he made to the conception of national policies locally administered, which underlies much of British government still, was great. Had his public career ended there, he would surely be remembered (if at all) as a minister of competence and energy, stiff in manner and lacking in personal charm, but a steady and sensitive social reformer, an effective parliamentary performer, and a British patriot of the first order. But "fate" had more in store for him. The final decade of his life will be treated in a second volume, in which the author's talent for balanced judgment will be tested more severely.

WILLIAM R. ROCK
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ROGER SAWYER. *Casement: The Flawed Hero*. Boston: Routledge and Kegan Paul. 1984. Pp. xv, 199. \$25.00.

Hero because of a distinguished career in the British consular service, but flawed because of his execution for treason in 1916, Sir Roger Casement turned out to be by far the most notable traitor arraigned and punished in the United Kingdom during either world war. In this full, well-reasoned, and singularly just biography, Roger Sawyer attempts to reconstruct the complete man or, rather, the complete set of parts, for he contends that Casement was never a truly coherent, whole person. Violently contradictory social, religious, political, and, above all, familial pressures set him firmly on the road to ultimate self-destruction.

Given that these pressures were combined with Casement's highly active homosexual proclivities, Sawyer's contention is plausible. And yet Casement's seemingly incurable immaturity was there anyway—at the very most aggravated by environmental strains that were sufficient to swing him over to high treason. Whatever the truth of things, he found it easy to slip away from reality into the wild, romanticized fantasies of "drawing room" Irish nationalism, even while pursuing a consular career, which

zoomed along a high plateau of sustained success. Here, clearly, was a highly weird individual.

Casement had a series of bewildering experiences as a child, which led him to a destination a good deal odder than that of a more usual soul. The son of a mixed Anglo-Irish, Protestant-Catholic marriage, Casement was cast into the "cold North" of County Antrim, where his paternal uncle had a country house. The United Kingdom accelerated Casement's advance in 1895 by waiving his need to take the usual examinations, and, therefore, his need to pass them. The long stretches in consular posts from Mozambique to Brazil, where he necessarily endured a second-class role vis-à-vis the Foreign Office men, could well have bred bitterness, but Casement moped not. On the contrary, he took up his task and walked all over the lesser diplomats while keeping the confidence if not of kings then at least of one foreign secretary, Sir Edward Grey. Far from a frustrating experience, the chance to expose oppression in the Congo and large parts of Brazil and Peru afforded a highly ambitious man like Casement truly high "ego satisfaction." His knighthood in 1911 is the most important proof of this, but so, however, was the way that his reports dismayed and shook the "mighty."

In such circumstances, feelings of being slighted by the British government would have figured far less in prompting Casement's treason than a growing *folie de grandeur*, a vitally important element in first supporting the independent, republican Ireland cause and then conceiving of Germany as the instrument whereby to bring it success. Where one fantasy might flourish, so might another. Casement's conversion to Roman Catholicism was doubtless genuine enough, but frenetic and much bound up with his pursuit of Irishness. After all, his adopted South Americans were to be not only Catholics but Irishmen as well! While not mad within the then grossly unscientific Common Law definitions of insanity, Casement was hardly fit for a ride on the "Clapham omnibus." Although this is not averred by Sawyer, it shouts out from his pages loud and clear.

MICHAEL HURST
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Oxford University

RUDOLF KLEPSCH. *British Labour im Ersten Weltkrieg: Die Ausnahmesituation des Krieges, 1914–1918, als Problem und Chance der britischen Arbeiterbewegung*. Göttingen: Traugott Bautz. 1983. Pp. 375.

This book's central question—"Could labor have made greater gains in World War I?"—is old, its tale familiar, its moral trite. But the documentation—fuller and wider than almost any other book's—is

new. Rudolf Klepsch's footnotes and quotations are good; from them, or at least in their presence, he draws an entirely predictable verdict.

Facts and footnotes count in history, we like to think. But, of course, it is presupposition that reigns supreme. What one assumes to be true makes all the difference. "Presupposition" here means choosing between David Lloyd George and G. D. H. Cole from the past, between Henry Pelling or Ralph Miliband among today's scholars, choosing between the modish New Left (that is, the stalest Old Left) assumptions or the opposite, less trendy premises. Did Lloyd George and Herbert Asquith trick the Labour party? Were Arthur Henderson and other leaders mere dupes, willing dupes, or bought and ambitious careerists? Did real alternatives for greater militancy and success exist but fail through leaders' faults? To all these but one, Klepsch says, "Yes": Labour leaders were duped, were vain, but were not calculating traitors. Klepsch does not seem to have considered other hypotheses, such as that Britain's life was at stake, that workers saw this, and that, as Englishmen, they therefore accepted every sacrifice. To me, these hypotheses are overwhelmingly closer to fact. Oh, for a muse of fire, a "sincrometer," and a magnet to help find objective truth!

The doctrinaires' prime crime is most often neither dogmatism nor abstraction but their chronic failure to expound and defend doctrine. Klepsch finally expounds his: short of violent mass revolution, limited chances existed for bold championing of class interests, and even these were lost through leaders' timid subservience, ineptness, and preoccupation with victory over Germany. Nonetheless, leaders, forced by mass pressure, turned halfway toward independence and criticism, but this was too little, too late (pp. 324-35).

Given Klepsch's heavy, if one-sided, documentation, two cautions apply. First, documents for the most crucial decisions *in camera* do not exist; for huge generalities such as Labour's "objective chances" for success, "documentation" could help little. Second, although he means to measure everything by the conditions and mentality of "the working class," Klepsch documents only the articulate decisions of inner circles. The nation, undocumented and unknown, stands outside.

PAUL BARTON JOHNSON
Roosevelt University

DAPHNE PATAI. *The Orwell Mystique: A Study in Male Ideology*. Amherst: University of Massachusetts Press. 1984. Pp. x, 334. Cloth \$30.00, paper \$14.95.

Not surprisingly, the year 1984 was marked by much attention to George Orwell and *Nineteen*

Eighty-Four, most of it laudatory. A notable exception is Daphne Patai's book, a brilliant and sustained feminist attack based on the premise "that the essential ideology at the heart of Orwell's work as a writer and thinker can be understood only by exploring his ideas about masculinity and femininity" (p. 1). From Patai's work emerges a devastating portrait of a limited and offensive writer whose reputation has been used to support the least attractive aspects of our culture. Just as one of Orwell's contributions was to force us to look about ourselves in new ways, so too Patai's book forces us to examine easily held assumptions about one of the most influential writers of our time.

Patai allows for the charge of anachronism (p. 5). Yet she judges Orwell more on our own contemporary concerns rather than on contextual evidence. Patai convincingly destroys Orwell's image as an honest, decent writer. But, even if Orwell himself contributed to the creation of the Orwell myth through his power to manipulate words, did not Lionel Trilling and others contribute as well? One has only to read Orwell to know that he was capable of the most outrageous and misleading generalizations and that he was full of hatred. Patai points out his extravagant generalizations and fully documents his nasty attitude toward women. He is also characterized as being anti-Catholic, anti-Semitic, and homophobic as well as opposed to crankiness and eccentricity, although he was in many ways a crank and an eccentric himself. Patai does not allow—not that this is an excuse for Orwell—that these are common attitudes among the English. Unfortunately, in some senses, these views may be part of his appeal as a writer. He was not afraid of stating what many believe to be "true." Through language he suggested that his real beliefs were in the English humane traditions, no matter how his actual words might present the contrary.

To have a discussion of Orwell from a feminist point of view is a considerable contribution. But it at times serves to distort perspective. I do not believe that the question of gender roles is the most important issue in *Nineteen Eighty-Four*. Although it is useful to have the importance of *Homage to Catalonia* as a masculine rite of passage pointed out, we read it to find out about aspects of the Spanish Civil War and, perhaps, about the nature of war itself. In general, the manifest and extremely important political and historical content of his writings and his genuine literary and intellectual accomplishments are not given their proper due.

Patai has an odd relationship to Orwell as a creative writer. She claims that he misinterprets the material that he presents to us. She does not sufficiently allow for the fact that he is an artist who chooses what he is going to put down on the page in a more creative way than a reporter. Patai has

argued in favor of a compelling countermyth to the "honest" and "decent" anticommunist that took shape in the 1950s. In so doing, she forces us to reexamine Orwell, and for that we are grateful. No one seriously interested in Orwell should fail to read this book. But the "real" Orwell may well have eluded both the "mystique" being attacked here as well as the dominating conception of this study: that he "cares more for his continuing privileges as a male than he does for the abstractions of justice, decency, and truth on behalf of which he claims to be writing" (p. 266).

PETER STANSKY
Stanford University

JAMES LEES-MILNE. *Harold Nicolson: A Biography*. Volume 2, 1930–1968. Hamden, Conn.: Archon. 1984. Pp. xii, 403.

This second and concluding volume of James Lees-Milne's authorized biography of Harold Nicolson begins in 1930, after Nicolson resigned from the Foreign Office to embark on a journalistic and literary career. As Lees-Milne explains, Nicolson was driven to work hard by ambition and the need to earn money to maintain his "aristocratic-bohemian" life style. He wrote a popular weekly column on current social and literary topics, published as many as five book reviews weekly, and delivered frequent public lectures and talks on the BBC. He was continually engaged in writing new books, turning them out with amazing celerity. Three of his best-known works—*Lord Carnock*, *Peacemaking 1919*, and *Curzon: The Last Phase*—were published between 1930 and 1934!

Nicolson was uncertain, however, that he would achieve greatness as a writer, and he therefore sought to make a name for himself in politics. In the early 1930s he joined Sir Oswald Mosley's New party, became editor of *Action*, and ran unsuccessfully for a seat in Parliament. In 1935 he was invited to contest West Leicester as the National Labour candidate, and with Conservative support he narrowly won the election. Nicolson's growing concern about the international situation led him to favor resistance to the Italian and German aggressors and to support critics of appeasement. He was rewarded when Winston Churchill, on taking over from Neville Chamberlain in May 1940, appointed him parliamentary secretary to the minister of information. In July 1941 the prime minister removed Nicolson from this post for political reasons but, as consolation, appointed him to a five-year term on the governing board of the BBC. Nicolson loyally supported the Churchill government until the July 1945 general election, when he and many colleagues lost their seats. He then applied to the new Attlee

government for a peerage, shocked family and friends by enrolling in the "Socialist party," and even ran—unenthusiastically and unsuccessfully—as the Labour candidate in a by-election at North Croydon (March 1948).

A few months later he finally abandoned all political ambition when he agreed to write the official biography of the king's father. Nicolson's *George V: His Life and Reign* (1952) is his longest and best book. It earned him favorable reviews from critics, sizable royalties, and also a knighthood. For the remainder of his long and productive life Nicolson continued his multifarious writing and speaking activities, served on important cultural commissions, and concerned himself with public and family affairs. Honored by governments and universities, he died in 1968, in his eighty-second year.

This is an informative volume, which will interest both the student and the general reader. As might be expected, it closely parallels and borrows from Nicolson's famous *Diaries and Letters, 1930–1962* (1966–68). Lees-Milne presents considerable fresh material, however, drawn from unpublished family papers, his own first-hand knowledge as a family friend, and various other sources listed in the endnotes and bibliography.

SYDNEY H. ZEBEL
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M. R. D. FOOT. *SOE: An Outline History of the Special Operations Executive, 1940–46*. London: British Broadcasting Corporation. 1984. Pp. 280. £8.50.

This book exhibits the strengths and weaknesses of M. R. D. Foot's previous writings on British relations with resistance movements. He is the author of what until recently was the only official history of the Special Operations Executive (*SOE in France* [1966]) and has also coauthored a history of the escape organization MI9. He is thus well-qualified to deal with the organizational and technical aspects of the SOE, as he does effectively in the first seven chapters. These deal with the origins of the SOE; the guerrilla wars it fought within the jungle of wartime bureaucracy; its command structure, recruiting, and training; and the gadgetry of secret war, communications, and security and penetration. For the general reader—and this book was designed to tie in with a recent BBC television series on the SOE—these chapters do a good job in communicating the texture of the SOE experience and in bringing to life some of its personalities. Foot is especially good when discussing recruiting, training, and the experiences of agents in the field. He has talked to many of those who survived, has in some cases read their

personal files, and was himself in Brittany with the Special Air Service during 1944. He thus has an excellent feel for this dimension of secret warfare. The cumulative effect is to bring home the distance between the ideal of an operation and its often messy or incomplete execution.

But politics was also an integral part of the SOE because its actions were dictated by the requirements of British strategy and diplomacy as they evolved over time. Here the book is weak. Foot tells us that the SOE training staff took the line that agents were sent abroad to perform strictly military tasks and should avoid political entanglements. Hence, agents who sought political guidance seldom found it and "were forced back on their own common sense" (p. 156). The reader can sympathize with the agent here, for Foot's political guidance is minimal at best. He is clearly uncomfortable with the ethnic and ideological dimensions of European politics. Nor does he really give us a history, for there is little sense here of any evolving strategy or policy governing SOE activities. There is little sense either of the grand strategy that provided the context for the SOE or of the SOE's own strategy in particular countries. What, one asks, was the meaning of the many exploits he recounts? And in the one chapter discussing where the SOE operated the section on Italy, to name but one example, is quite inadequate. Some basic books on Yugoslavia and Greece do not appear to have been consulted (they are not cited and there is no bibliography). The author says he has no intention of summarizing the official history of the SOE in the Far East, so we learn nothing about it. Perhaps we are expected to use our common sense.

It would be wrong to leave the book on this note, for it has other merits than those of its early chapters. Not the least is its avoidance of the currently fashionable anachronism with respect to Allied relations with the Soviet Union. The author recognizes both Russia's importance in winning the war and the dimensions of the communist wartime resurgence. He also sensibly dismisses ideas that Soviet agents in the SOE manipulated policy in the Balkans. He knows far too much about the SOE and has too much respect for history to fall for these tempting simplifications.

DAVID STAFFORD
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Toronto, Ontario

HARRY A. MISKIMIN. *Money and Power in Fifteenth-Century France*. (Yale Series in Economic History.) New Haven: Yale University Press. Pp. x, 303. \$26.00.

Most discussion of monetary and so-called real forces centers on how slow or rapid rates of increase in the money stock may affect the economy. Historically this discussion takes off from the expansion of Europe's money stock, which began just before the discovery of precious metals in the New World and gained momentum thereafter, a point first raised by Earl J. Hamilton and still of much interest. Harry A. Miskimin examines these issues, analyzing not so much the economic as the political and religious effects of a diminishing supply of bullion in France in the fifteenth century. He concludes that the shortage, significant enough to be classified as a bullion famine, assisted the development of Gallicanism. But even more it aided the growth of royal power toward absolutism. The French called on their kings to deal with the monetary scarcity. Even when the intention was to dismantle the repressive rule of Louis XI, as was the case in the Estates General of 1484, the result was enhanced royal authority over trade, consumption, and property and an undermining of the development of representative institutions.

These arguments are lucid and compelling. Miskimin is careful to remind the reader that he does not intend to claim that monetary forces alone had these effects, merely that monetary forces must be integrated into more conventional explanations of these developments. What remains at issue is the importance of the bullion scarcity, which emerged in the 1360s and, in Miskimin's view, deepened thereafter until the end of the fifteenth century. Since that was also an era of catastrophic population decline—the century of plague—how did the per capita supply of precious metals develop? Europe's aggregate bullion supply declined, but for what reasons? The need to ship precious metals, especially silver, to the East must be seen in terms of the relative magnitude of trade in comparison to output and the still smaller quantities represented by current account deficits. Perhaps the leading reason for the shrinkage of the aggregate bullion stock was wear and tear, a matter of considerable importance in an age when coins had a high precious metal content. Another factor that makes it difficult to establish the scale of the monetary effect is the role of recoinage. If the output figures from French mints (compiled by Miskimin in extended tables with data from printed and archival sources) are to serve as an indicator of the bullion stock, then coins reissued with a different, usually lower, silver or gold content have to be treated separately from coins struck for the first time. Furthermore, what portion of the record is represented by the mint output tables? Aware of these and other problems, Miskimin carries the discussion as far as is warranted by the state of the evidence.

On one matter, however, Miskimin's argument is not wholly persuasive. He holds that an examination of wheat prices shows that markets adjusted "virtually instantaneously" (p. 67) to debasements so that commodity prices were actually figured in bullion rather than in the livre of account, which was subject to alteration by the king. But the price data—often only a few wheat quotations per year—and the chronological structure of the test—year-to-year prices figured in bullion values—belie the claim of nearly instantaneous adjustment. Surely it was in the weeks or months of movement toward congruence that French kings found the rewards that encouraged them to continue practicing debasement.

Monetary forces, specifically the scarcity of bullion in fifteenth-century France, can now be considered with other factors in explaining political, religious, economic, even social, developments of that era. Yet, despite its quantitative nature, monetary evidence cannot be used to portray the scale of the effect quantitatively; fixing the scale of the monetary effect, like fixing the scale of other effects, requires the qualitative judgment of the historian.

JAMES C. RILEY
Indiana University

ROGER G. LITTLE. *The Parlement of Poitiers: War, Government, and Politics in France, 1418–1436*. (Royal Historical Society, Studies in History Series, number 42.) London: The Society; distributed by Humanities, Atlantic Highlands, N.J. 1984. Pp. xii, 251.

In the spring of 1418 the Burgundian faction seized control of Paris, and the dauphin, the future Charles VII, fled to the south where he attempted to govern France, first in the name of his insane father and from 1422 in his own right. As Paris and the remainder of northern France remained under English and Burgundian control, Charles had to create his own governmental institutions. Among them was a Parlement, which sat at Poitiers from 1418 until the recapture of Paris in 1436. There have been a number of studies of various aspects of this period, including the Parlement, and of Charles's reign as a whole. Roger G. Little's goal is to exploit the archives of the Parlement more fully than any of his predecessors and to use the material "to provide a reassessment, not only of the creation, organisation and function of the *Parlement* itself, but in a broader sense of the enigmatic early years of Charles VII's reign" (p. 1). Unfortunately, except for the archives of the Parlement, he relies entirely on published sources and secondary works. Thus, he neglects the archives of the towns when dealing with their relations with the Parlement. In a chapter on the recruitment, functions, and wealth of the *Parlementaires* we are told who they were, what they

did, and what income they received in so far as it appeared in the parliamentary records. We learn little of their social background or other possible sources of income. The author includes none of the charts or tables so dear to the *Annales* school and little information for the growing number of students of the patron-client relationship.

There are, however, admirable aspects of this book. Little's research is careful and his interpretations provocative. His style is straightforward and clear, although one could wish that he had translated the numerous French quotations that break up the otherwise smooth-flowing narrative. The historical revisions he offers merit consideration, although in some instances he goes too far. He finds that "the period 1418–36 marked a dynamic phase of Charles's reign, characterized by ceaseless journeying" (p. 58). The role of Joan of Arc in the recovery of France is correspondingly reduced. The Parlement at Poitiers was not feeble and its magistrates were not poor. The several thousand refugees who fled to the south led to "the creation of new industries and the expansion of existing ones" (p. 212). The charge that Charles was weak and apathetic in his dealings with the nobility is rejected. Rather, his "reign constituted one of the last phases in the centuries-old attempt by the French crown to control the independent ambitions of its greater vassals and to bring them more closely into its own orbit" (p. 212). "Charles's lavish concessions to the nobility . . . displayed great foresight [because he] needed every ally he could get. . . . Hence, conciliation and concession rather than punishment and alienation are themes which occur again and again. . . . [Charles as a monarch had] an acute grasp of the political realities of his time" (p. 213).

J. RUSSELL MAJOR
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MARIO TURCHETTI. *Concordia o tolleranza? François Baudouin (1520–1573) e i "Moyenneurs."* (Travaux d'Humanisme et Renaissance, number 200.) Geneva: Droz. 1984. Pp. 649.

This challenging but most welcome book is various things. It is a new and another rehabilitating study of the once-curious François Baudouin, long considered the chameleon of the Reformation. It is equally a dispassionate and ecumenical study of the middle or irenic party of the Reformation, of which Baudouin was a significant part, in the critical period 1550–70. Finally, if incidentally, its voluminous documentation constitutes virtually a parallel book, which provides a sensitive, rather exhaustive, and sometimes exhausting review of the battle of the historians that has inevitably succeeded that of the history itself. No doubt, I will not be the only one to

turn to the index of this book with apprehension as well as anticipation.

Mario Turchetti's major objective is to clarify once and for all the difference between the two terms of the title, concord and toleration. Descending from Erasmus, the great men advocating concord are Baudouin, George Cassander, and Claude D'Espence. Apart from the *politiques*, the great proponent of a definitive toleration is Sebastian Castellio. The two parties have often been indiscriminately rolled into one, but the author argues consistently and convincingly that the proponents of concord championed only a tactical or temporary toleration as a condition of ultimate unity. They could not countenance the dismemberment of Christendom. Turchetti's principal thesis is that "the theory of concord of Baudouin is not to be confused with the various theories of toleration" (p. 445), which seems more than adequately demonstrated.

Turchetti also demonstrates greater continuity and stability in the sometime "chameleon" of the Reformation. Accused of changing his religion seven times in twenty years, Baudouin actually gradually returned to his original Catholic faith. The consequence was some of the sharpest polemics of a polemical age. Baudouin had to deal with not just his old master, Calvin, but also Calvin's successor, Theodore Beza, who considered that Baudouin had rejoined "the apostates of Satan" (p. 595 n. 69). Calvin comes through here as less ecumenical than in some earlier studies, including my own, with a distinction noted between the earlier and later Calvin. He eventually turned against concord as betraying the Gospel. Baudouin, for his part, was directed to the Fathers by the cardinal of Lorraine and primarily Lorraine's *peritus*, D'Espence (finally getting some much merited attention but still wanting a monograph). Armed with Augustine and Optatus, Baudouin used the latter's work against the Donatists as a "blast of the trumpet" to tar Protestants with the brush of Donatism (p. 481).

The "crucial moment" and centerpiece of the book is the Colloquy of Poissy and its sequel, the Edict of Toleration of January 1562, representing, respectively, concord and tolerance (pp. 592–93). In this I am naturally pleased to see that my own work on Poissy has been largely sustained. The author is perspicacious about its greatest controversy, the matter of the cardinal of Lorraine, but Turchetti generally continues the rehabilitating scholarship of the late H. O. Evennett. He even finds new evidence in support of Lorraine's sincerity. At the same time he concludes that Nicola Sutherland's revisionist essay against both Evennett and myself is "difficult to sustain" (p. 222 n. 53).

Apparently the maiden effort of an obviously very promising scholar, this book is profusely documented, brilliantly integrated, and gracefully writ-

ten. Typographical errors are minimal for a work of this magnitude, but, unfortunately, one of them is that Richard Popkin has somehow become H. Hopkin (pp. 99, 614).

DONALD CHRISTOPHER NUGENT
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PHILIP T. HOFFMAN. *Church and Community in the Diocese of Lyon, 1500–1789*. (Yale Historical Publications, Miscellany, number 132.) New Haven: Yale University Press. 1984. Pp. x, 239.

Philip T. Hoffman's study of the implementation of the Counter Reformation in the diocese of Lyon concentrates on relations between the Catholic clergy and parish communities in town and country. To understand the long-term development of Catholic reform, he has chosen to study the entire period of the *ancien régime*, thus revealing several interesting trends that might otherwise have escaped notice. The major themes of his discussion follow the arguments of Robert Muchembled, Jean Delumeau, and Peter Burke that the Counter Reformation represented an attack by a urban elite culture on a rural popular culture and religion. Hoffman studies first the resurgence of Catholicism in the city of Lyon, where a new spirit of piety set in during the second half of the sixteenth century, overcoming the old lay hostility to the clergy and allowing the laity a role in civic politics previously denied them. From this secure urban platform a renewed Tridentine clergy was able to launch an assault on the religious practices of the countryside at the beginning of the seventeenth century.

Hoffman provides a succinct overview of the state of the rural clergy and its role in the ritual and communal life of the parishes during the sixteenth century. He then discusses the nature and extent of clerical reform, which was to separate clergymen from close contact with their parishioners, turn them into exemplars of a new piety and spirituality, and imbue them with hostility to the old morality, rituals, and celebrations of the countryside. This was a piety inspired by the individualistic, sober, disciplined ethic of the urban elites, but one that sought to take away lay control over religious life and invest it firmly in the hands of the clergy. Here the clerical thrust of reform could even attack earlier lay forms of Catholic reform, such as some of the sixteenth-century confraternities. It was an ethic of self-control that emphasized the sharp separation of the sacred and the profane, condemning the world of the flesh as the realm of Satan. Measured in terms of schools, charities, confraternities, pious bequests, and the suppression of popular festivals and rituals, clergymen were more or less successful in their task—nowhere more so than among women, to

whom the new individualistic piety offered more spiritual opportunities than the old male-dominated forms of collective religion. But the clergy experienced resistance throughout the course of the seventeenth century, and by the second half of the eighteenth Hoffman is able to point to considerable popular hostility to the reformed clergy. Ironically, the same movement of reform that made them a more educated, cohesive professional body also isolated them from their rural parishioners on the eve of the revolution.

Hoffman analyzes these developments with economy and ease of style. His discussion is based on a broad range of local archival material, including wills, criminal records, and the records of diocesan archives. He makes intelligent and convincing use of quantitative analysis and sampling techniques, carefully laying out the limitations of his sources yet managing imaginatively to extract valid long-term trends from them. He cogently establishes the applicability for his diocese of Muchembled's "two cultures" theme, although he has less to say on Muchembled's notion of acculturation (perhaps wisely, given the criticism the concept has attracted). Because of the terms of reference of his study, we see little of the further argument that such a reform movement served the interests of the absolutist state. Nor do we hear much about the attack on popular magic, a singular omission, since we are not told whether this was simply not reflected in the sources or was not an issue in Lyon. Nonetheless, this book is skillfully and intelligently written; full of interesting material, it is a signal contribution to our understanding of the wider dimensions of the Counter Reformation.

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JAMES LOWTH GOLDSMITH. *Les Salers et les d'Escorailles: Seigneurs de Haute Auvergne, 1500–1789*. Translated by JACQUES BUTTIN. (Publication de l'Institut d'Études du Massif Central, number 25.) Clermont-Ferrand: The Institute. 1984. Pp. 239.

This is an impressive study. James Lowth Goldsmith has analyzed—with considerable ingenuity and a large range of documents—the functioning of two Auvergnat lordships over the Old Regime. The result is an excellent contribution to the history of an important and poorly known group: the provincial military nobility, those families that enjoyed sufficient prosperity to play a role in provincial politics but that stood far below the great courtly aristocracy.

Like other historians, Goldsmith sets the history of the nobility within a context of enduring agricul-

tural stagnation: in the Auvergne, in fact, stagnation persisted to the 1960s. Also like others, Goldsmith emphasizes the nobles' economic success within this setting. Most of the nobles aggressively pursued revenues and undertook investments in land, barns, and livestock that assured their leadership of the provincial economy. Seigneurial rents remained important throughout the Old Regime. In this region there was no late medieval "crisis of feudalism"; throughout the period seigneurial revenues counted for about one-third of the total revenue of the estates that Goldsmith studies. But these nobles also depended heavily on dairy production for the market. They were not mere rent collectors.

Goldsmith suggests that the main variables affecting the nobles' prosperity lay outside the realm of production altogether. Such difficulties as the nobles encountered resulted from personalities and political forces rather than from change in the economy itself. Personal incompetence, occasionally visible despite imperfections in the record, was one source of trouble. Much more significant, however, was the burden of political and social pretension: costly marriages and other expenses were required to maintain a visible role on the provincial stage.

But the most important source of difficulty came from the state of Louis XIV. Goldsmith's analysis of this interchange between the state and the seigneurial system is the most original part of his book. During the *Grands Jours* of Auvergne the crown dispatched royal commissioners to the region to repress aristocratic violence, and they seized a number of aristocratic properties. This was sufficiently serious by itself, but it also unleashed hatreds throughout local society. Resentful bourgeois undertook lawsuits against noble enemies, efforts that might be extremely damaging when these bourgeois held royal offices. The crown's efforts also stimulated widespread peasant resistance to seigneurial dues, in the form both of litigation and rent strikes. In the Auvergne, Goldsmith shows, the seigneurial system rested on a precarious balance of force. When Louis XIV upset that balance, popular hatred of the seigneurial system came immediately and effectively to the fore.

Goldsmith's treatment of the eighteenth century, when this damage was made good, is less satisfying because much briefer. Eighteenth-century changes in nobles' culture and economic practice are briefly taken up but not fully analyzed. One may complain as well of Goldsmith's readiness to generalize from the experience of a very backward province in his conclusions about the seigneurial system. The Auvergne formed part of that "other France" whose development was very different from that of the northern plains.

This is nonetheless an important book. Much in it will be of interest mainly to specialists; Goldsmith

proceeds methodically and in great detail through the workings of the early sixteenth-century lordship. But the conclusions that this methodical analysis permits—about continuities in rural social structure and about the politics of lordship—should interest all students of early modern France.

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NATALIE ZEMON DAVIS. *The Return of Martin Guerre*. Cambridge: Harvard University Press. 1983. Pp. x, 162. \$15.00.

It is a fitting tribute to a leading American social historian of early modern France that she has helped shape a French film version of, as well as written a monograph on, the celebrated sixteenth-century story of Martin Guerre. Moviegoers can turn to Natalie Zemon Davis's book for answers to many questions the film avoided about the strange case of Arnaud du Tilh, adventurer, reprobate, and ex-soldier for France: how he masqueraded for three years as Martin Guerre—misfit, wife deserter, and soldier for the Spanish side—and supposedly returned from the Franco-Spanish wars to his wife, Bertrande de Rols, and to the Languedocian peasant community of Artigat, not far from the Guerre family's original Basque homeland.

Davis has brought to life this strange and unusual story that was the talk of sixteenth-century France, a story so compelling that it has been revived on and off ever since. It is a natural subject for a late twentieth-century social historian, because it brings into bold relief the aspirations, feelings, relationships, and personal maneuverability of female and male peasants from a society whose secrets are rarely unsealed. This is precisely Davis's rationale for retelling the tale, and in a short review one must concentrate on that aspect.

The first thing one notices is the amazing amount of archival sleuthing packed into every page of this little book. The author has combed the local archives for information on the male principals and on the contrast between the familial restrictions on property alienation in the Guerre family's Basque homeland and the more conventional property arrangements of Artigat, which the false Martin Guerre tried to impose on the real Martin's uncle (then remarried to Bertrande's mother and head of an extended household). The author has also delved into Bertrande de Rols's world, its marriage customs and social constraints, where peasant women had little room to maneuver and where the particle "de" did not mean the peasants were aping nobility but instead signaled the legal and social attachment of female peasants to males in their

family. In Bertrande's case, she was de Rols while her father was simply Rols. She was, in fact, subordinated first to her father, then to her husband, and finally to her widowed mother and her uncle, turned stepfather!

Arnaud's opportunistic rescue of Bertrande from that maze, his tragic hanging for the crime of acting as a deserted woman's husband that neither his crypto-Protestant nor staunchly Catholic judges knew how to handle, and his feigned life in between have enough universal appeal to be the focus of the film version. But Bertrande's clever, dogged, double game of having her cake and eating it within the confines of a particularist, male-dominated society is the natural centerpiece of Davis's less romantic but more realistic and brilliantly scholarly monograph. If the author occasionally turns conjecture into reality ("Bertrande dreamed of a husband and a lover who would come back, and be different" [p. 34]) and at times leaves us wondering whether she is following closely the two original contemporary accounts of the Martin Guerre story or drifting into the "perhapses" of her contextual analysis (compressed footnoting may account for much of the confusion), she has surely given us a splendid example of mature social history.

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PHILIP JOHN STEAD. *The Police of France*. New York: Macmillan. 1983. Pp. xii, 178. \$19.95.

Philip John Stead has written an institutional history that parallels the major political events of French history from Louis XIV to the present. The work serves as a useful general reference to the nature and operations of the modern police in France. Although its chronological divisions include the *ancien régime*, the revolution, the Napoleonic period, the nineteenth century, and the period from 1900 to 1945, this book is best at explaining reforms and developments after World War II and at describing how the contemporary system works. It provides lucid descriptions of such complex procedures and operations as the criminal justice system and transcends the focus of policing to include an informative discussion of the training of judges, their competence, and their independence.

Stead writes in the tradition of Henry Buisson and Marcel LeClere, both police historians and police officers, whose writings he admires. Faithful to that tradition, Stead highlights the central role of police administrators, and the result is a kind of in-house history. He regards his subjects with admiration and, at times, even awe. As an example, LaReynie, one of Louis XIV's lieutenants of police, is praised as "a great Parisian" (p. 16) and "a great

pioneer in the art and science of policing the big city" (p. 21). Yet Stead's generally favorable orientation leads him in some cases to expect too much of the French police. In 1789 "[p]olice failure to chart the current of public opinion and to present it to the king and his ministers must nevertheless be held against them" (pp. 32–33). Even with the assumption that the police must be held accountable for negligence on the eve of the revolution, this is a work of much praise and little blame.

Above all, the work is one of admiration for the centralized and "monolithic" French system, which Stead favorably contrasts to the locally controlled policing organizations of the United States and Great Britain. "There is no getting away from the fact that the police is more important in France than it is across the Channel or across the Atlantic" (p. 162). Both the state and the citizenry look to the police for protection. Its omnipotence is, of course, also its vulnerability.

Stead misuses the institution of the police to understand social events, as is exemplified by his treatment of May 1968. By focusing on police actions, Stead concludes that the event was the result of a misunderstanding. A "routine police procedure" of "shepherding" students into buses to check their identity cards was misunderstood by bystanders who "started to attack the police" (pp. 104–05). In the six weeks that followed, the police "stood the test of unprecedented stress and strain" (p. 106), kept channels of communication open to the students, and performed in a "highly creditable" fashion (p. 108). Little here illuminates why the participants in the May events perceived the police as a central enemy or why May 1968 occurred at all and lasted as long as it did, other than that it was set off by "one of those incidents that, misunderstood, provoke fierce reaction" (p. 104). In looking at the May events only from the perspective of the police, Stead's conclusions deal with little more than advances in deployment principles since the February riots of 1934.

Finally, Stead favorably judges police recruitment and training, especially for the Gendarmerie Nationale, which "combines military and police science to generate professional excellence" and is "a model of what can be achieved by training when government commits the necessary resources to it" (p. 143). Stead's study is, on the whole, a paen to the "high quality in personnel and performance" of the French police (p. 163). He does little to help us understand the development of the police in relation to social change in the modern period. Yet, by highlighting the unique role and importance of the French police, although unfortunately doing so in terms of justification and praise, Stead is reminding

historians that the police were not shadows but actors in the history of modern France.

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STEPHEN HOLMES. *Benjamin Constant and the Making of Modern Liberalism*. New Haven: Yale University Press. 1984. Pp. vii, 337. \$27.50.

One might think that the shaping of modern liberalism has been fairly well charted and that Benjamin Constant's contribution to it has been adequately described by several authors, among them Paul Bastid (1966) and Guy Dodge (1980). Stephen Holmes has, nonetheless, given us the most impressive account of Constant's political thought as it was elaborated through a tumultuous career from the French Revolution through the Bourbon Restoration. Holmes's approach is highly analytical rather than narrational. He begins with chapters on liberty and freedom (chaps. 1–2) and Rousseau (chap. 3), proceeds to the "political psychology" of mistrust and hypocrisy (chap. 4), "constitutional design" (chap. 5), romanticism (chap. 6), progress (chap. 7), and tyranny (chap. 8), and ends with the "counterenlightenment" (chap. 9) and public and private interests (epilogue). Each chapter is subdivided into six to ten sections devoted to themes such as "Romantic militarism" (p. 165), "public apathy as a countervailing power" (p. 112), and "civic privatism and its problems" (p. 43), which show how much Holmes has transcended the historical limitations of his subject. Constant effectively serves as a springboard for a liberal analysis of terror, dictatorship, traditionalism, and, above all, liberty. Although Holmes indulges in biography sparingly, his book is no abstract commentary on liberalism but is based on solid documentation and peppered with informed and sharp perceptions about the French revolutionary and imperial experience.

The most alluring idea of the book, one treated before by Dodge and others but perhaps not so well, is Constant's juxtaposition of ancient and modern liberty, in which the first is repudiated as a model for the second (chaps. 1–2). Ancient liberty consisted of maximum participation in the public life of the *polis*. A private sphere of personal liberties, religion, and rights hardly existed. Modern liberty had to guarantee the individual an impenetrable sector of his own—that of the family, morality, religion, and free thought and expression. The failure of the French Revolution, from this liberal perspective, lay largely in its attempt to use a classical model of liberty, when society had, since the Reformation (according to Constant), developed a private sphere, ill-served by civic festivals, denunciations, and surveillance committees. Far from envisioning a republic of transpar-

ent sincerity like Rousseau, Constant countenanced individual hypocrisy as a legitimate stratagem of survival in regimes of terror and despotism.

Holmes's exposition mimics a bit what he observes in his subject. Both move through "palinodes" and "palimpsests," the retracting or qualifying of what has been written or the rewriting it in altered form. Almost every issue on which Constant pronounced had to be modified as historical circumstances and Constant's ideas changed. Rousseau is criticized for having unwittingly provided "a mask of virtue" behind which private vengeance hid during the revolution (chap. 3), yet in the same chapter Constant's attachment to the "general will" and the rights of man enables Holmes to write that "Constant in many respects considered himself a devout Rousseauist" (p. 86). Likewise, Constant's antidemocratic reputation is severely qualified by attention to Constant's attachment to public liberty, which provides an antidote to the vice of "privatism." The Terror is unambiguously assailed by subject and critic, but Constant is clearly separated from the traditionalists (Bonald and De Maistre), who alone get a thorough drubbing, in part because Constant, unlike they, saw the revolution as irreversible.

The author has made as strong—even as brilliant—a case for Constant and liberalism as can be made. The shortcoming of the book lies in that strength: one wishes for more critical distance on Holmes's part from the subject and less defensive admiration. Dodge's much shorter treatment succeeds better in relativizing Constant's views in a liberal nineteenth-century context. Holmes's acknowledgement of Constant's limitations are drawn largely from outside his subject's *expérience vécue* (capitalism and industrial working conditions). Other problems go unnoticed, like the difficulties that arise either in achieving or after having achieved a radical separation of public and private spheres (for example, in family morality). One misses some of the harsh verdicts contemporaries passed on Constant; his brilliant tergiversation led Jeremy Bentham to quip, "*sola inconstantia constans*." Constant was one of many adept survivalists of the age of revolution. One wonders whether this adeptness has not also some intimate connection with liberalism. Making allowance for the author's conviction, an incomplete index, and endnotes rather than footnotes, one can recommend this book as extremely intelligent, refreshing, authoritative, and physically attractive.

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DONALD R. KELLEY. *Historians and the Law in Postrevolutionary France*. Princeton: Princeton University Press. 1984. Pp. 184. \$22.50.

The school of Romantic historiography that flourished in France during the first half of the nineteenth century has been subjected to intensive scholarly scrutiny from a variety of perspectives. After Hayden White's and Lionel Gossman's "internalist" examinations of literary texts and Stanley Mellon's "externalist" analysis of political contexts, not to mention any number of monographs on Jules Michelet, Edgar Quinet, François Guizot, and the rest, is there room for yet another contribution to the literature? Donald R. Kelley persuades us that there is with this slim, unpretentious volume. It is less a systematic analysis or narrative account than a thoughtful essay by a respected specialist in an earlier period whose first foray into the nineteenth century has yielded an aspect of Romantic historical writing that has hitherto eluded the experts.

Whereas most standard studies of the subject have emphasized the Romantic historians' interdisciplinary fascination with the methods and perspectives of literature and philosophy, Kelley identifies an equally important link to the dominant school of jurisprudence. The Romantic historians' obsession with resurrecting the distant past coincided with the objectives of the historical school of law that had emerged in Restoration France. A shared conviction that the basic patterns of a particular society could be uncovered by an examination of its legal arrangements forged the link between Clio and Themis, the goddess of human law.

This partnership between historians and legal scholars was strengthened by an ideological affinity as well. Friedrich Karl von Savigny's denunciation of the Napoleonic Code as a misguided attempt to impose a priori, rationalist principles on a society governed by custom and sentiment struck a responsive chord in Restoration scholars of history and jurisprudence alike. Judicial authority was celebrated by both schools as an indispensable source of continuity and conservation: interpretation adapted laws enacted by the people's representatives to the concrete requirements of society. In this way law came to be regarded as the genuine expression of social reality and legal scholarship as the principal means of addressing "the burning issues of the age," such as the historical origin and evolution of the institution of private property.

One is entitled to question the wisdom of Kelley's decision to ignore the major figures in his attempt to restore the reputation of the likes of Henri Klimrath, A. J. Pardessus, and Charles Giraud. He himself acknowledges that such forgotten figures "may deserve their obscurity" (p. 27). Moreover, it remains an open question whether he has discovered a genuine alliance between the "new history" and the "historical school of law" or simply identified a simultaneous development of two separate intellectual movements. To record that Louis Adolphe

Thiers, François Mignet, and Augustin Thierry were all lawyers by training or to repeat Montesquieu's dictum that "one must illuminate history by laws and laws by history" scarcely clinches the argument. Nonetheless, this modest investigation of a previously unexplored cranny of nineteenth-century French cultural history merits the attention of specialists in the subject.

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RICHARD HOLMES. *The Road to Sedan: The French Army, 1866–70*. (Royal Historical Society Studies in History, number 41.) Atlantic Highlands, N.J.: Humanities or the Royal Historical Society, London. 1984. Pp. vii, 272.

The collapse of the French army in 1870 was so utterly spectacular that contemporaries searching for a similar instance when such a highly rated army plummeted so swiftly to defeat were immediately reminded of Jena. Then, as now, many were prepared to say that the Second Empire got the army it deserved. Hardly was the ink dry on the Treaty of Frankfurt than the incompetence of a "comic opera high command," the treachery of Marshall Achille François Bazaine, and the decadence of France itself were cited as just a few of the complications to which the patient finally succumbed.

In this detailed and excellent study Richard Holmes examines what has too often been dismissed as "the painted army of a painted emperor." He reminds us that the French forces on the eve of Sedan were neither decadent nor lacking in courage; Algeria, the Crimea, and the Italian campaign of 1859 had established them as the world's premier army. This army, however, was a force designed for fighting small wars and maintaining internal order. Officers with foresight had recognized how tenuous many of its victories, especially those of 1859, had been, even before the thunderstroke of Sadowa announced that *esprit militaire* and *furia francaese* might in the future be no more than quaint attributes useful for soldiers engaged in bushwacking Arabs.

Holmes identifies six major weaknesses in the army that marched out to meet Helmuth von Moltke in July 1870: faulty organization and administration, ideological unpreparedness for total war, an aging officer corps that was "a closed corporation of poor men," low standards of education and training, numerical inferiority, and a high command that hovered somewhere between complete senility and mere old age. On this last point, Holmes is a trifle kinder to "les Africains" than contemporaries and historians have tended to be, concluding that their influence in the army has been exagger-

ated and that their performance, even if far from adequate, was nevertheless superior to that of artillery and staff generals. Holmes does not do for French Commander in Chief Bazaine what John Terrain has attempted for Douglas Haig—that is, excuse shortcomings so extraordinary that taken together they constitute a fairly complete list of indictable offenses. He merely reminds us that Bazaine was passed a "poisoned chalice" whose contents would have set the great von Moltke himself writhing in agony. The most damning charge that can be laid at the unfortunate marshal's feet is that he was foolish enough to drink from it.

Placed in its historical context, of course, the debacle of the *année terrible* comes as no surprise. There was enormous complacency in the army before 1870. But there was also a recognition, even in the imperial household, that a root-and-branch reform was the only way to match the Prussian build-up. The obstacles to reform, however, were virtually insurmountable: the lack of agreement among reformers, political opposition from the Left, savage cuts in the military budget, and the realization on the part of Napoleon III that a frank admission of French military weakness might well scupper the regime. The death of Marshal Niel removed the only man capable of producing an effective compromise out of a complicated mix of political and military questions. After all, even in Prussia, that land of "an army which had a country," it took the combined talents of Bismarck, von Moltke, von Roon, and the Kaiser two wars, a constitutional crisis, and about four years to railroad military reforms through the Prussian Diet. In the end, the Second Empire did indeed meet the fate it deserved, able neither to avert war nor to prepare for it.

DOUGLAS PORCH
The Citadel

SANDRA HORVATH-PETERSON. *Victor Duruy and French Education: Liberal Reform in the Second Empire*. Baton Rouge: Louisiana State University Press. 1984. Pp. xvi, 278. \$30.00.

Victor Duruy, the French minister of public instruction from 1862 to 1869, has always been highly regarded by historians of France. This reputation has not rested primarily on concrete achievements. Although he effected a variety of minor technical improvements at all levels of education, he was responsible for only two major innovations, neither of which was an unmixed success: the creation of the *École Pratique des Hautes Études* and the setting up of a special secondary education system as an alternative to classical studies.

Duruy's reputation, I would argue, rests on his tireless activities, which both expressed and helped give form to a growing consensus among teachers throughout the system that education at all levels needed to be reformed. He translated vague critiques into practical proposals, gave them a documentary basis through the studies he commissioned of the French and foreign educational systems, and helped organize them into a coherent program of liberal reforms that was taken up by republican moderates after 1870. He succeeded at this level because he was perceived by academics as one of themselves, committed to education rather than to partisan politics or the interests of the Catholic church.

Sandra Horvath-Peterson tells the story of Duruy's busy and controversial years in office with competence and occasionally with flair. She goes into far greater detail about Duruy's policies than earlier historians, and, although she adds few genuinely new insights, she nicely fills the gaps in our existing knowledge. Telling the entire story in a single, well-organized volume will certainly be useful to specialists.

Despite these virtues, the book has several flaws that weakened its impact, at least on me. Horvath-Peterson sometimes allows her obvious admiration for Duruy to approach hero-worship. She occasionally sounds like his eulogists, from whom, incidentally, she quotes liberally. She tends as a result to overstate his achievements, claiming, for instance, that his minor curricular reforms of classical studies "added up to a pedagogical revolution" (p. 148). (This evaluation would have surprised reformers of the 1870s and 1880s for whom Duruy's measures had changed very little.) One reason I find her judgments questionable is that she does not display a very deep knowledge of events before and after Duruy's ministry and overlooks a good deal of recent secondary literature on various aspects of French education in the nineteenth century. She refers to only a single article by C. R. Day, even though two others bear more directly on her chapter concerning special secondary education. The work of Terry Shinn, Victor Karady, and many others, which is scattered throughout numerous periodicals, has been used only in part, if at all. Much of this literature applies only indirectly to Duruy's ministry. But it is, nonetheless, indispensable to a full appreciation of Duruy's overall achievement.

More serious, Horvath-Peterson is not strong on theoretical issues. She spends several pages early in the book, for instance, discussing the deficiencies and weakness of French public education on the eve of Duruy's appointment (pp. 48–51). Throughout, she assumes that these were objective, totally unproblematic entities and not arguments marshaled

by specific groups for whom current arrangements were no longer satisfactory. In her conclusion, to mention another example, she cites my own citation of a book by Michel Crozier and E. Friedberg in a 1980 article, in which I suggested that Crozier had moved away from the extreme views expressed in *The Bureaucratic Phenomenon* and was now prepared to admit that incrementalism had on occasion led to substantial reform in France. Horvath-Peterson, however, uses the citation to support the following statement: "From the beginning to the end, therefore, Duruy was a proponent of the only reform that has truly succeeded in France—incremental reform" (p. 246). This is less an example of misrepresentation than of a general difficulty in handling more abstract issues.

Overall, therefore, the usefulness of this book lies less in its judgments or interpretations than in its careful descriptions of the policies, actions, and strategies of a major figure in the history of French education.

GEORGE WEISZ
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LENARD R. BERLANSTEIN. *The Working People of Paris, 1871–1914*. (Johns Hopkins University Studies in Historical and Political Science, 102d series, number 2.) Baltimore: Johns Hopkins University Press. 1984. Pp. xvii, 274. \$25.95.

Since historical research has increasingly become the "testing" of narrow hypotheses, we should welcome this book by Lenard R. Berlanstein. The author has painted a convincing and richly textured picture of the working people of Paris and its suburbs between the Paris Commune and the First World War. He rejects the simplistic alternative theses that workers became either more integrated into or more alienated from industrial society. Rather, he finds a diverse working class, which he persuasively divides into five groups (artisans, specialized hand workers, factory laborers, service workers, and low-level white-collar workers), each with distinct experiences and prospects. He clarifies their diverse social origins, patterns of sociability and residence, working conditions and traditions, living standards, social expectations, and modes of protest and political behavior. And he presents this vast analysis in a thoroughly readable style with economy of expression.

This book helps explain the material and cultural differences that underlay the inability of socialists and unions to create a cohesive working class before World War I. Berlanstein shows not only the social and cultural divisions between craft-oriented Paris and the industrial suburbs and those between white-collar and industrial workers but also the gulf that

separated the perspectives of service and factory workers, despite their similar income and skill levels. This book complements studies on working-class life earlier in the nineteenth century, since the author traces the fate of skilled artisans and the more humble specialized pieceworkers at the end of the century. Berlanstein also introduces us to the new Parisian office employee and the suburban auto worker. He avoids the temptation to find radical transformations where the evidence indicates only gradual change.

This book also challenges some widely held assumptions about Parisian workers in this period, namely, the *embourgeoisement* of the city, the docility of the clerk, working-class adaptation of middle-class family patterns, and the successful rationalization of the automobile and other new industries in the suburbs. The analysis of living standards goes beyond the measurement of income and housing to a consideration of diet, health, regularity of employment, and work time. The treatment of work, the impact of technology, and industrial discipline are also balanced and subtle. Berlanstein's chapter on leisure and family life catches glimpses of the pleasures of suburban factory workers, documents the emergence of the vacation for clerks, pinpoints the decline of fertility of working-class families, and shows us working-class concern with adolescent gangs and the emerging teenager problem.

Berlanstein's study successfully presents a multi-dimensional history of Parisian workers over these two generations. To be sure, the reader may not always find clear analysis of trends from, for example, serial data on wages or strikes, and the major turning points—though obviously different for each group and for each level of analysis—could be less ambiguous. We also wonder why Berlanstein chose to confine his social history to a political-historical time frame. He does, however, draw the useful conclusion that this period, so often assumed to be decisive in the history of French labor, is hardly a dramatic deviation from the historical flow. Yet readers may find that Berlanstein is perhaps too modest. He clearly shows patterns in the transformation of the work and life of labor that inform our understanding of European workers both before and after this period.

GARY CROSS
Pennsylvania State University

JOLYON HOWORTH. *Édouard Vaillant: La création de l'unité socialiste en France; La politique de l'action totale*. Foreword by MADELEINE REBÉRIOUX. Paris: Études et Documentation Internationales or Syros, Paris. 1982. Pp. 378. 98 fr.

This work by Jolyon Howorth reminds us that in the well-ploughed history of pre-1914 French socialism, to which Anglo-American scholarship has contributed its fair share, the contributions of Édouard Vaillant to socialist theory and practice have not commanded the attention they merit. In the eyes of the French working class and European socialists, Vaillant was every bit as important as Jean Jaurès or Jules Guesde. A *communard*, he spent a decade of exile in London before returning to France, where he served for nine years on the Paris municipal council and then in 1893 began an uninterrupted parliamentary career that ended only with his death in 1915. In the socialist movement his major accomplishment was to help bridge the ideological and practical differences that splintered French socialism and blocked the creation of a unified party.

With the failure of the Paris Commune the appeal of nineteenth-century insurrectionism à la Blanqui had faded. The republic opened up new channels of opportunity through the ballot, political organization, labor unions, reform legislation, and parliamentary action, but it also created new dilemmas, some of which emerged with the Dreyfus affair and others with Alexandre Millerand's entry into the cabinet in 1899. Less a biography than an extended analysis of Vaillant's political philosophy and tactics, this monograph explores the subtle way in which Vaillant evolved his philosophy of "total action," combining parliamentary activity with other means of struggle and seeking a balance between revolutionary theory and pragmatic politics. He often sided with Guesde, the uncompromising champion of class struggle, but his devotion to the parliamentary republic was more consistent than that of Guesde. His differences with Jaurès are especially intriguing. At times he saw Jaurès as defending with excessive zeal the cause of individual justice, as in the Dreyfus affair, and joining too readily with Radicals in coalitions and blocs, thus losing sight of the larger socialist goal. He rejected Jaurès's defense of ministerialism, siding with the SPD-dominated Second International on that issue. Yet it was a tribute to Vaillant's skill and patience that on many matters, including the condemnation of ministerialism, he eventually won Jaurès over to his viewpoint and made party unification possible. That unification merely cloaked persistent differences hardly needs pointing out.

In the last decade of his life, as the French delegate to the socialist International Bureau, he bravely but in vain worked to avert war, a firm believer, like Jaurès, that working-class action (socialist and syndicalist combined) would serve as an effective barrier. At the outbreak of hostilities, however, Vaillant, like other French socialists, threw himself into the war effort: socialism would have to await the end of German militarism. A more ex-

tended analysis of this turnabout than is provided here would have been welcome.

Although comprehensive and discerning, Ho-worth's monograph, a translation of an unpublished dissertation originally written in English, contains little on the socialist or syndicalist rank and file, the concerns of working-class life, or even the turbulence within Vaillant's Federation of the Seine. But it is nonetheless an able assessment of Vaillant's contribution to the never-ending balancing act that French socialists from Louis Blanc to François Mitterrand have had to perform between reformism and revolution. Vaillant's ambivalent prescription, it appears, was "revolutionary evolutionism."

JOEL COLTON
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De Gaulle et la nation face aux problèmes de défense, 1945-1946. (Colloque Organisé par l'Institut d'Histoire du Temps Présent et l'Institut Charles-de-Gaulle, 1982.) Paris: Collection Espoir, for the Institut Charles-de-Gaulle or the Institut d'Histoire du Temps Présent. 1983. Pp. 317. 110 fr.

The systematic study of contemporary history has become one of the main areas of scholarship in France. Its prime movers are not the historians of the *Annales* school, but young academics trained or inspired by René Rémond and situated primarily at the Fondation Nationale des Sciences Politiques and at François Béradida's Institut d'Histoire du Temps Présent. In the fall of 1982 this institute and the Institut Charles de Gaulle, a center devoted to research on the General's life, works, and thoughts and headed by some of his former associates, organized a colloquium on the issues of defense after the liberation of France, during the period of provisional government that preceded the establishment of the Fourth Republic. The bulk of the discussions dealt with the de Gaulle phase, from the liberation until the resignation of the General in January 1946. This volume contains the papers and a transcript of the debates. The participants were military men (who wrote many of the papers and had, in several instances, taken part in the events described) as well as former public officials and academics.

The first "reign" of General de Gaulle remains less well known than either the Fourth Republic or the Gaullist phase of the Fifth. This volume contributes to our knowledge in one important area, but it is as frustrating as it is interesting, and not solely because the reports frequently overlap and the discussions, with a few exceptions, add more anecdotes than deep insights. There are two serious deficiencies. First, little is said about one of the most troublesome and controversial issues faced by de Gaulle's new government: the integration of the

Resistance fighters into the regular army that had landed in Normandy and in Provence. Second, both de Gaulle's decisions about military affairs and the stands taken by the political parties about defense were essentially the by-products of more fundamental concerns: de Gaulle's overall diplomatic design and views about France's role in the world, the parties' own domestic priorities, and notions about who should be responsible for national security—the head of the executive branch or the legislators. Many of the papers—particularly those by Jean-Pierre Rioux and Loïc Philip (the son of de Gaulle's main challenger of 1945, the Socialist André Philip)—touch on these decisive issues. But, given the colloquium's focus, what was primordial in 1945-46 remains peripheral here. De Gaulle always wanted defense to be subordinated to (high) politics; a symposium on defense thus risks being too narrow.

This does not mean that we learn little. The colloquium deals, on the whole, with three subjects. One is the deadly contest between the ambitions of the postliberation military and the economic and financial constraints necessitated by the condition of France after four years of occupation, exploitation, and destruction. The sweeping plan for twenty divisions drawn up by de Gaulle in June 1945 had to be drastically curtailed in September, again in December, and again after de Gaulle's departure. The second subject is the growing "misunderstanding" between the nation and the army: after a brief honeymoon, many Frenchmen began to feel that the new armed forces were excessively wasteful and demanding, and the political parties—the old, like the Communist and Socialist parties, or the new, like the Christian Democratic M.R.P.—decided that economic reconstruction had to be given precedence over the ambition of building up and equipping strong military forces. Hence, in the last months of 1945 a head-on collision with de Gaulle raised the fundamental issue: who should govern, the assembly or the General? The mix of these two issues caused his abrupt resignation, even though a compromise had been found on the more limited issue of a Socialist amendment reducing military expenditures by 20 percent, which had led to a tense debate in the constituent assembly in December 1981.

The third subject is the role of de Gaulle. The colloquium confirms what one knows of his views on Germany (which he wanted to dismember) and on Britain (which he accused of fomenting trouble for France in Syria and Lebanon—quite unfairly, as François Kersaudy incisively shows). De Gaulle's determination to put an end to dependence on allied powers for the equipment of France's forces and his will to restore or preserve French authority in the empire, from Indochina to North Africa, by

force if necessary, are also confirmed, as, however, is the uncertainty about his long-range political expectations and plans for overseas areas, especially for Indochina (de Gaulle's son-in-law, General de Boissieu, presents him as more liberal than many other students of the issue would suggest). What is very clear is de Gaulle's interest in nuclear affairs and his intuition about the importance of atomic power in the future; hence, his decision to establish the Commissariat à l'Energie Atomique. Equally clear is his will to remain in control of defense policy. Far less clear is how he thought he would balance his own ambitious goals and France's domestic constraints: he understood that economic growth was a prerequisite for grandeur but could not dissociate grandeur from a strong army present in every continent. He wanted the army to be capable of intervening abroad quickly but resigned himself to a military service of one year only; the issue of how much money should be budgeted for the armed forces was unresolved at the time of his departure. One author notes that he did not show much interest in setting the policy to be followed by France's army of occupation in Germany. Nor did he seem much more interested in the air force than before the war. What emerges is similar to what Michel Debré, one of de Gaulle's most loyal associates, states in the recently published first volume of his memoirs: the General reserved key decisions to himself in this period but otherwise delegated a great deal of responsibility and often seemed to observe (and sometimes deride) his subordinates rather than command and coordinate them. Things would be different in many respects after 1958.

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DONALD D. HORWARD. *Napoleon and Iberia—The Twin Sieges of Ciudad Rodrigo and Almeida, 1810*. (Florida State University Book.) Tallahassee: University Presses of Florida. 1984. Pp. xx, 419. \$29.50.

Donald D. Horward's work, the first detailed account of the twin sieges of Ciudad Rodrigo and Almeida, is based on a thorough examination of the archives of the four powers involved in the Peninsular War and particularly the Archives de Messéna. The result portrays in great detail the two sieges, conducted by the French Army of Portugal. These operations are depicted from the considerations of the top level command down to company and patrol detachments. The detail is important, for it conveys clearly the difficulties with which the French forces had to cope. The problems of logistics and subsistence are very evident in these pages, as are the great exertions needed to maintain a large army in a region largely denuded of resources. Also, the many

factors that strained the relations of the top French officers are discussed.

Horward's narrative takes the reader right into the trenches. One gets a feeling for the mechanics and techniques of the siege warfare of the time. The impact of such operations on the morale of the troops on both sides is made very clear.

The work is not confined simply to an account of the operations that form the core of the book. Horward places them in the overall setting of the war. He deals extensively with Wellington's problems, the reasons for his decisions in relation to the sieges, and the considerations governing the actions of the commanders of the two fortresses.

The text is enhanced by numerous illustrations, principally of the fortifications and other key features that figured in the sieges. The maps enable the reader to follow the operations very well. There is a glossary to assist the reader who is not familiar with all the technical terms of fortifications and siege warfare. Finally, there is a very good index.

Horward has written an excellent example of operational military history. It adds considerably to the knowledge of the operations of the Peninsular War and shows the importance of the sieges of Ciudad Rodrigo and Almeida to the outcome of the 1810–11 campaigns in Portugal. Horward correctly contends that Wellington considered these defenses vital and relied on a good defense of each fortress to gain the time necessary to complete the Torres Vedras lines. For this reason the aborted siege of Almeida caused Wellington considerable concern. Offsetting this judgment, however, are Napoleon's instructions to André Masséna. After ordering the two sieges, Napoleon directed Masséna to "prepare to march methodically into Portugal, which I do not wish to invade until September, after the hot weather and particularly the harvest." These imply that the French would not have advanced earlier than they did in any case. All in all, Horward has written an excellent work with which all students of the Napoleonic Wars need to become acquainted.

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JOHN F. COVERDALE. *The Basque Phase of Spain's First Carlist War*. Princeton: Princeton University Press. 1984. Pp. ix, 332. \$31.00.

In recent times Spain, which was one of the more peaceful lands of Western Europe during the early modern period, has been the classic country of civil war. Three civil wars (the number differs according to the individual historian) between liberals and traditionalists in the nineteenth century were followed by Europe's most widely publicized civil war of the twentieth century between revolutionaries

and counterrevolutionaries. Although Spain has one of the longest liberal traditions in Western Europe and the world, it has also contained the most persistent traditionalists. Their most sustained effort, made in the nineteenth century, is the focus of John F. Coverdale's new book.

As is the case with most major developments in modern Spain, a sizable literature dealing with the First Carlist War exists, but it mostly consists of frequently partisan, nineteenth-century publications. In recent years scholarly attention has resulted in rather narrow monographic studies of the war. Coverdale does not attempt to cover the entire conflict but, rather, thoroughly examines the background and early development of the war and its primary phase in Navarre and the Basque country from 1833 to 1840. Although a full account with the same objectivity, depth of research, and attention to detail would obviously have been even more useful, this book fills a major gap. The first 120 pages present the most lucid study in any language of the development of the conflict between liberals and traditionalists and the background to the war. The main section deals with the crucial first two years of the war in which the traditionalist cause took full political form. The result is a solid, new treatment that is unrivaled in the literature and makes the whole contest clearly accessible to students and nonspecialists for the first time.

It is based on wide-ranging investigation in the Basque, Navarrese, and Spanish national archives as well as thorough reading in the memoir and secondary literature. The exposition is fair and impartial and is particularly effective in explaining the regional Basque-Navarrese context and its differences from the social, economic, and institutional structure of the rest of Spain—differences that made this region the primary bulwark of traditionalism not merely in the 1830s but also for a hundred years afterward. Coverdale describes the infrastructure of the Carlist war effort in its political, economic, and organizational dimensions and gives a trenchant analysis of the relationship of the traditionalist cause to the regional social structure, the problem of the *fueros*, and the Catholic church. He assesses the precise role of each and attributes the strength of Carlism in the Spanish northeast to no one facet alone but to the integration of diverse interests in the defense of an entire way of life. His conclusions are convincing and a useful corrective to the various monocausal interpretations and distortions advanced by diverse Spanish writers. The last chapter, moreover, provides an excellent analytical summary of the final four years of the war in Spain as a whole, which will be quite useful to students. The most obvious technical shortcoming is the absence of a clear and accurate military map, since the nineteenth-century chart provided at the beginning

of the book is not adequate. This, however, detracts but little from the utility of a study that will now become the standard reference for the background and the first part of the First Carlist War.

STANLEY G. PAYNE
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COLIN M. WINSTON. *Workers and the Right in Spain, 1900–1936*. Princeton: Princeton University Press, 1985. Pp. xv, 361.

The restoration of democracy in Spain has given rise to an outpouring of works on Spanish labor history. The majority have concentrated on the socialist *Unión General de Trabajadores* (UGT) and the anarcho-sindicalist *Confederación Nacional del Trabajo* (CNT); few have discussed the rise and fall of Catholic syndicalism. The traditional interpretation of the *Sindicatos Libres*, founded by the ex-Carlist militiaman Ramon Sales, is that they were little more than groups of desperadoes and strikebreakers in the pay of the Barcelona employers' association. These "yellow" unions did not achieve lasting success; in 1933, at the height of left-wing organization, the UGT and the CNT each boasted more than one million members, but the National Confederation of Catholic Workers estimated its own membership at a mere thirty-six thousand. In this scholarly and well-written account, Colin M. Winston rejects the interpretation of the Spanish Left and calls for a new approach to labor historiography. Based on archival research, mostly in Barcelona, his study traces the development of right-wing trade unionism from radical Carlism to proletarian fascism.

Before 1909 the labor movement in Catalonia was relatively tame and nonthreatening. Catalan anarchism, divorced from the organized proletariat, retreated into violence, and in the infamous "tragic week" of 1909 arsonists indulged in a spate of convent burning that destroyed one-third of Barcelona's ecclesiastical property. Fearing social revolution, the Catholic establishment of the principality supported a brand of confessional trade unionism known as *Acción Social Popular* (ASP), founded by the Jesuit Father Palau. Winston charts the ASP's lack of progress and demonstrates its subordination to Spanish capitalism, which condemned the organization to blackleg status. Meanwhile, Carlism, originally a reactionary movement whose strongholds were in the rural interior, enjoyed a remarkable revival in Barcelona. Radical Carlism exalted violence and in 1911 founded shock troops to take part in street fighting with the anticlerical Young Barbarians. By 1919, when Sales founded the *Libres*, this hatred was redirected against the anarcho-sindicalists who had plunged the region into a de facto state of civil war.

Winston discerns three distinct periods in early *Libre* history. From October 1919 to the first months of 1921 the new body was eclipsed by a rampant CNT, against which it cooperated with sectors of the employers' federation. Until October 1922 the *Libres* enjoyed extensive official protection from the civil governor of Barcelona, Martínez Anido, sent to the province at the request of Catalan big business to butcher the CNT. For eighteen months, Winston argues, the *Sindicatos Libres* became virtually the sole representatives of organized labor in Catalonia. Thousands who had not the slightest connection with Carlism poured into the *Libres*, including former members of the banned CNT. Yet, when Martínez Anido was dismissed in the autumn of 1922, the *Libres* proved incapable of competing with a resurgent CNT.

Winston's contention is that, although members of the *Sindicatos Libres* frequently acted as scab labor, it would be misleading to characterize them as fundamentally a "yellow" or "company" union. If the employers' association saw Sales as their lackey, he saw himself as a true representative of the workers, less militant than the *cenetistas* but still critical of industrial capitalism.

When General Miguel Primo de Rivera overthrew the politicians in September 1923, the *Libres* quickly arrived at a *modus vivendi* with the new regime. Entrusted with participation in Primo's labor schemes, the *Libres* took on a number of fascist traits, of which they had already been accused as early as 1922. Fortunately, the overthrow of the dictatorship brought about their early demise.

Winston's detailed and challenging study is convincing to the extent that the *Sindicatos Libres* are now provided with an identity of their own. No longer can they be dismissed as merely a gang of "righteous" thugs and assassins in the employ of big business. Whether his plea for a new approach to labor historiography is taken up by others is quite a different matter.

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E. H. CARR. *The Comintern and the Spanish Civil War*. Edited by TAMARA DEUTSCHER. New York: Pantheon. 1984. Pp. xix, 111. Cloth \$14.95, paper \$7.95.

This is a posthumously published work by E. H. Carr, the renowned English historian of Soviet Russia. According to the introduction, the great merit of the book is that it provides "a new perspective on the complexity of motives which lay behind Stalin's policy [in Spain] and on the intricate relationship of forces in Europe of that period" (p. xviii). Unfortunately, this is not the case. Carr's conclusion that

Moscow's attitude toward Spain was dictated less by *raison de la révolution* and more by *raison d'état* is hardly new. It was reached by many at the time and has been amply confirmed by such historians as David Cattell, Burnett Bolloten, and Stanley Payne. The same is true for the conclusions that the tactical objective of the Comintern and the Soviets was to exercise control over Spanish affairs and that the Spanish Communist party was a willing and docile instrument of Soviet authority.

The book also seems to ignore the findings of much recent scholarship on the Spanish war. This leads to numerous errors or ambiguities on a range of subjects. For example, Franco's role as the original leader of the rebellion is overemphasized, as is the degree of defection from the army to the rebellion. The implication that the rebellion was eagerly welcomed in Berlin and that Hitler enjoyed the prospect of creating a fascist state in Western Europe is inaccurate. The assertion that the adoption of the "mixed brigade" as the structural basis of the new "people's army," and the introduction of the political commissar system into that army, stemmed from the example of the International Brigades (IB) is incorrect. The explanation of the origins of the IB is also erroneous, as is the implication that George Orwell was a member of that organization. And, although correctly allowing that European Communist parties were instructed by the Comintern to recruit for the IB, it is not made clear that the IB was a direct product of the Comintern and that its political directorate was made up exclusively of such Comintern stalwarts as Andre Marty, Luigi Longo, and Franz Dählem.

Finally, in discussing the waning of zeal in support of the Loyalist cause by 1937, Carr says, "The revolutionaries of many nations who had marched to the defense of Madrid in November 1936 found no corresponding inspiration in the defense of Azaña's bourgeois republic" (in 1937) (pp. 45-46). The odd thing here is that Azaña's "bourgeois republic" was much more a thing of the past by 1937 than it had been in November 1936. Perhaps the zeal had been sapped by disillusionment with what the Soviets and their Comintern agents were doing in Spain.

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A. SMITS. *1830: Scheuring in de Nederlanden*. Volume 1, *Holland stoot België en daarmee Vlaanderen af*; volume 2, *Brussell met Wallonie veroveren Vlaanderen, zetten zich uit tot België en stoten op hun beurt Holland en daarmee ook de Generaliteitslanden af* [1830: Schism in the Netherlands. Vol. 1, Holland Rejects Belgium and Also Flanders; vol. 2, Brussels and Wallonia Conquer Flanders and Expand to Become Belgium

and, in turn, Reject Holland and the Generality Lands]. (Anciens Pays et Assemblées d'États, number 83.) Heule, Belgium: UGA. 1983. Pp. 432; 509.

"The historical observer never confronts a bare and simple fact unrelated to a broader historical context," said Johan Huizinga in *The Idea of History*, because "history is unthinkable without theory." In this book, however, A. Smits has revealed that history is entirely thinkable with no "theory," on the one hand, and too much "broader context," on the other. In two volumes totaling over nine hundred pages, the author has presented a dazzling array of fascinating facts about the actions of King Willem I, his ministers, and governors, as well as of the opposition forces in the southern provinces, culminating in the bifurcation of the Kingdom of the Netherlands, initially created by the Congress of Vienna. The rupture that took place in 1830 ended the reunion of north and south, eventually producing the modern nation-states of Belgium and the Netherlands. This is undoubtedly a central event in the history of the Low Countries that could benefit from a fresh analysis. Smits should be commended for the massive quantity of interesting details he has unearthed about the behavior of individuals who contributed to the political, economic, and cultural tensions within the kingdom. His admirable accumulation of qualitative data, however, has not provided novel insights into the origins of the revolution of 1830 in Belgium.

Once the kingdom had been established as a political entity in 1815, King Willem I confronted major obstacles in unifying the economically and culturally diverse provinces. A southern fear of economic favoritism of the north persisted, in spite of the generous state investments in Belgian industry and infrastructural roads and canals that helped connect both "sides" of the kingdom. The semi-official predominance of the Dutch Reformed Church in the north, which was also the *locus* of political authority, generated anxiety among both the Catholic hierarchy and laymen in the Belgian provinces. The question of control over education was paramount, and language policy continued to be a well-spring of discontent. Large groups of French-speaking bourgeois, trained and matured under the Napoleonic scepter, felt disenfranchised in a political structure in which the *lingua franca* was Dutch.

Given such wide-ranging and deep-seated discontent, an explosive situation may have been inevitable. The crucial question, however, is why the eruption occurred at the very moment it did, and with such irrevocable consequences. As Smits points out, the protest movements and petition drives of the late 1820s—orchestrated by a "monster" alliance of Francophile lawyers, liberal journalists, southern aristocrats, and devout Catholics—were quite suc-

cessful: practically all "Belgian" complaints were met with a desired change in policy, resulting not only in greater educational freedom but also in linguistic liberty. Why, then, did this very success generate crisis and formal political separation?

It is at this level of analysis that Smits's corpulent volumes fail because too many different lines of argumentation are pursued, all, in a curious way, revealing his peculiar political biases. In his review of the "causes" of the revolt, for instance, he argues that the Belgian separation crisis was a social rebellion—a part of the fallout from the "glorious" events in Paris—fostered by the growing mechanization of industry, increases in food prices, and a drop in market demand. In itself such a causal explanation might be plausible, but Smits omits a focused discussion of the actual relationship between the material suffering of the poor and the political uprising in the southern provinces. Instead, he suggests that the ponderous and slow Willem—a well-intentioned if misguided enlightened despot who lacked the ability to make firm decisions—was responsible for the crisis. Smits compares Willem I to four previous members of the House of Orange by stressing similar examples of alleged indecisiveness. The revolt was fueled as well, he argues, by the wish of the Belgian Catholic clergy to reconstitute itself more or less as the First Estate. The French-speaking bourgeoisie in Flanders, according to Smits still in a feudal relationship with the pauperized Flemish people, also played a crucial role by refusing to support a unified state. Furthermore, such regressive political developments, when combined with the demands of liberals for ministerial accountability, compounded the desire in Wallonia for political autonomy or, *in extremis*, unification with France. According to the author, Wallonia was aided and abetted by a Paris depicted as an *agent provocateur* in the political drama of the Low Countries. Smits provides even more explanations, all equally interesting and mostly plausible. But in the end his mind-boggling set of arguments produces a classic case of confusing the forest with the trees.

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MARIANNE SCHAUB. *Müntzer contre Luther: Le droit divin contre l'absolutisme princier*. Paris: A l'Enseigne de l'Arbre Verdoyant, with the cooperation of the Centre National des Lettres; distributed by Armand Colin, Paris. 1984. Pp. 399. 130 fr.

Marianne Schaub approaches the Müntzer-Luther controversy over the perfectibility of society from the standpoint of her interest in the influence this conflict has exerted on the political philosophy initiated by the German Enlightenment and developed

by Marx in the tradition of the German post-Enlightenment search for a native revolutionary tradition as opposed to the traditions of the French Revolution.

The first half of the book consists of three sections: a brief discussion of interpretations of Müntzer and the Peasants' War, a description and analysis of Müntzer's career, and "Müntzer's Theology and the Philosophical Significance of the Luther-Müntzer Debate." The second half of the book consists of French translations of three of Müntzer's writings ("The Princes' Sermon," "A Special Exposé of False Faith," and "Highly Provoked Defense"), Luther's tract against the peasants, the "Twelve Articles," "The Article-Letter of the Black Forest Peasants," and the "Program for Reform of the Empire." Except for Müntzer's second tract, these are all available in English. Schaub also includes here her thesis on "The Correspondence between the French and German Nations." The volume concludes with eight pages of exceedingly brief sketches of eleven Reformation figures. There is a short index of names and a bibliography.

At best, this volume makes no advance in Reformation scholarship. Those interested in the development of Marxist historiography would better invest their time in Abraham Friesen's *Reformation and Utopia: The Marxist Interpretation of the Reformation and Its Antecedents* (1974). Schaub appears to be unaware of more recent and theologically sensitive studies by East German Marxist historians such as Adolf Laube, as well as Peter Blickle's *The Revolution of 1525: The German Peasants' War from a New Perspective* (1981), the German original of which appeared in 1977!

At worst, this volume repeats the old charges first hurled by Müntzer that Luther interiorized and individualized freedom, sanctified feudal hierarchy, and legitimized princely authority. Thus, her subtitle is "Divine Right versus Princely Absolutism."

As during the sixteenth century, ideological polarization replete with denunciations of "evil empires" is not unknown to us. But neither the gospel of progress and social perfectibility nor Engels's idea that (German) history is a permanent state of misery is more viable today than then. Although Müntzer's totalitarianism was theocratic, his motto that "the godless person has no right to live" has, mutatis mutandis, a familiar ring. It was against this ideologizing of politics by the claim of divine righteousness that Luther stormed. Luther insisted that the construction of a better but never perfect society is possible only when persons are freed from arrogating divine right to their cause. Such freedom, he believed, was possible by faith but not by the sword. As Reinhold Niebuhr paraphrased this, "Man's capacity for justice makes democracy possible; but man's inclination to injustice makes democracy nec-

essary" (*The Children of Light and the Children of Darkness* [1960], p. xiii).

One hopes that French scholars of the Reformation will provide a more adequate study of the continuing significance of the conflict between Müntzer and Luther. A study of Müntzer comparable to Marc Lienhard's *Martin Luther: Un temps, une vie, un message* (1983) would be a good counterpoint to Schaub's book for the French-reading public.

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PAUL RAABE. *Bücherlust und Lesefreuden: Beiträge zur Geschichte des Buchwesens im 18. und frühen 19. Jahrhundert*. Stuttgart: J. B. Metzler. 1984. Pp. x, 344. DM 72.

This book brings together seventeen of Paul Raabe's essays on the history of books, publishers, and authors in the eighteenth and early nineteenth centuries. Raabe is one of West Germany's most distinguished research librarians, perhaps most widely known through his bibliographic guides for students of German literature; his interests as a literary historian have focused on the *Goethezeit*. The papers presented here have appeared over the past thirty years in various journals and scholarly collections, some not always easily accessible outside Germany, so this book now provides the most convenient and systematic approach to Raabe's work on the history of the book.

The range of his research and its development are instructive in themselves. The earliest pieces address individual authors—Schiller, Goethe, Hölderlin—and their relations with publishers, showing both aesthetic and pragmatic concerns in the making of books. From this initial interest in the practical life of German high literature, Raabe's perspectives broadened over the years to encompass more general issues of intellectual and cultural history. One of the most impressive papers in this collection uses the famous Berlin publisher Friedrich Nicolai's large catalogue of books in stock for 1787 as the source for a broad examination of German publishing: the diverse subjects and formats represented; the leading as well as minor firms and their lack of specialization; and, despite an impressive concentration in Leipzig and Berlin or in larger commercial cities over smaller residence or university towns, the decentralized character of the industry. Most of the remaining essays in the volume reflect this newer emphasis on print and publishers as agents of general cultural change over the eighteenth century.

"The Bookdealer in the Eighteenth Century," "The University and the Book Trade," "The Newspaper as Medium of Enlightenment," and "Book Production and the Reading Public, 1770–1780"—

such titles indicate Raabe's move toward a social history of literature. He addresses significant problems in these informative and readable essays. His enthusiasm is almost as infectious, I think, as his penetrating command of the sources is impressive. Yet the specificity that marks Raabe's empirical studies is sometimes combined with a puzzling willingness to make elusive or undemonstrated generalizations. His discussions of bookdealers as mediators between authors and readers provide recurring examples. Raabe takes much care with his writers and publishers yet also makes remarkable assumptions or inferences about the "broad" audience for enlightenment or the "democratization" of reading—without presenting any real evidence or even referring to the issue of literacy levels in the 1770s or 1780s, a period he identifies plausibly as the "takeoff" in the growth of German publishing. The argument is not strengthened by his unfortunate failure to make statistical estimates on the industry consistent from essay to essay. Social historians will sense such flaws in this suggestive and stimulating book most strongly. They will see it as a pleasing reflection of some new directions in literary history, will value it as an aid to finding sources and topics for future research, and will hope to better its treatment of readership. For all these things, Raabe will deserve their gratitude and respect.

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JONATHAN SPERBER. *Popular Catholicism in Nineteenth-Century Germany*. Princeton: Princeton University Press. 1984. Pp. xii, 319. \$45.00.

The place of the churches, and still more of religion itself, in the lives of the German people a century or more ago is as hazardous as it is difficult for the historian to assess and characterize. For this reason most studies of political Catholicism in pre-1914 Germany avoid discussion of sectarian belief as such. Historians either content themselves with analysis of religion as a cultural expression of material interests or prefer instead to restrict their focus to the parliamentary behavior of Catholic political leaders. Few of these writings seek to come to terms with mass Catholic politics and its relationship to both social change and changing forms of popular religion.

A notable and important exception is this book by Jonathan Sperber. Drawing on governmental and church records, Sperber not only describes a remarkable transformation in popular Catholic religiosity among the inhabitants of Westphalia and the northern Rhineland between 1830 and 1870 but also skillfully assesses (with the aid of quantitative

methodologies) the political implications of this renewal, from the revolutions of 1848–49 down to the bitter Kulturkampf of the 1870s. During the two decades before the mid-century revolutions the religious environment in this corner of the Hohenzollern kingdom was characterized by declining church attendance and the decay of traditional religious practices, devotions, and piety. According to Sperber, this process of secularization was in part the consequence of disruptive socioeconomic factors, in part the outcome of divisions within the Catholic community itself, and in part the result of governmental interference in religious affairs.

The two decades after 1850, on the other hand, witnessed a vigorous religious renewal. Fashioned and dominated by the clergy, this revival replaced the secularizing and morally lax tendencies of the *Vormärz* with an improved popular morality that was encouraged by means of missions, revival meetings, pilgrimages, and sundry other forms of religious expression and practice and was sustained within an institutional framework of new associations and organizations. This reshaping process, he argues, was facilitated by a mid-century subsistence crisis that impressed on the lower classes the need for greater sobriety and constraint, by the church's success after 1850 in adapting its organizational forms to meet the needs of rural migrants flooding into industrial towns, and, in the aftermath of the revolutions of 1848–49, by the absence of bureaucratic interference in church matters.

It was precisely this religious revival that explains the emergence between 1866 and 1871 of political Catholicism as the dominant force in Westphalia and the northern Rhineland. The mobilization of mass religious sentiment, together with the reorganization of political Catholicism as a coherent movement, led to important electoral victories in 1870 and again in 1871. These conclusions, Sperber claims, challenge traditional assumptions that locate the origins of Catholic political allegiance in the Kulturkampf of the 1870s. He argues instead that the church-state dispute must be understood as "an exacerbating or encouraging rather than an originating factor" (p. 286). Far more important than the Kulturkampf in forging Catholic political loyalties, he maintains, were the two decades of religious renewal that preceded the conflict.

Although Sperber has much to say about the nature and character of that devotional renewal, how it was made and sustained, and even why the Catholic populace in this region responded as readily as it did, his persistent and dedicated analysis tells us more about the form and the externals of this revived piety than about religious belief as such. His account also does not discuss important topics like the incidence (or absence) of spectacular conversion among prominent personalities, the place,

function, and contribution of the many varieties of Catholic religious orders in the area, or even the role of denominational schools in the making and consolidating of the religious renewal that remains at the center of his concern.

Some of these problems are no doubt inevitable in a pioneering study of this kind, and, overall, this book has undeniable value. Sperber's willingness to explore issues too often overlooked—popular religion, its practices, and its organizational forms—make this book an impressive contribution to the discussion of cultural response to those socioeconomic and political changes in Central Europe that accompanied the transition from the late preindustrial era to a period of industrialization and urbanization.

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REGINE QUACK-EUSTATHIADES. *Der deutsche Philhellenismus während des griechischen Freiheitskampfes, 1821–1827*. (Südosteuropäische Arbeiten, number 79.) Munich: R. Oldenbourg. 1984. Pp. 285.

Over the years the various aspects of philhellenism have not lacked chroniclers. Indeed, in *That Greece Might Still Be Free* (1972) William St. Clair produced a comprehensive, scholarly, and vivid treatment of philhellenism in its full European dimension. St. Clair emphasized the importance of pamphlets in promoting the Greek cause in Germany, where the fervor for the Greeks that followed the outbreak of hostilities in 1821 was stronger than anywhere else in Europe. He further noted that to get a full picture of what the pamphleteers produced is difficult, since so many of the brochures are hard to find.

Now Regine Quack-Eustathiades has chosen to focus on these pamphlets for a new study of German philhellenism. She stresses that, because of the way censorship functioned in the fragmented German Confederation, there tended to be more freedom of expression in pamphlets than in newspapers. She has tracked down some thirty-four pamphlets and gives full bibliographical details, including where and when they were reviewed. Since the nature of a pamphlet is to be rather brief, the total material on which the study is primarily based is thus not very large, but for her discussion she constructs a substantial superstructure in two parts. The first part, "Germany's Active Concern with the Greek War of Liberation," goes over fairly familiar ground. Its most useful section is a broadly based treatment of the reception at home of volunteers' reports of their experiences in Greece. (Until the end of 1822 the number of German volunteers far

exceeded that of any other country.) The second part, "The Political Writings on the Greek War," is the core of the book. The writings are discussed in chronological order, and a final section describes their main themes. Taking the book as a whole, the treatment involves a good deal of repetition; few readers, as the saying goes, are likely to find it too short. The main value is in the bibliographical details and the summaries of the various pamphlets, many of which would otherwise be inaccessible to most researchers.

The author has missed an opportunity to enhance her book's usefulness by not specifically addressing the role of literary periodicals in disseminating to a wider public the views expressed by the pamphleteers. Such periodicals were flourishing, and the extensiveness of their reviewing, well documented by the author, is astonishing. Also, the attitudes of such publishers as F. Brockhaus and J. F. Cotta, both of whom brought out important literary-political journals, would have been well worth a few pages. After all, it was as a series of articles in Cotta's *Allgemeine Zeitung*, not as a pamphlet, that Friedrich Thiersch published the views that were so influential in launching the philhellene effort in 1821. If the author had dealt with such matters in combination with a more rigorous streamlining of her material, her well-researched book would have been a more effective addition to the literature of the subject.

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H. I. BACH. *The German Jew: A Synthesis of Judaism and Western Civilization, 1730–1930*. (Littman Library of Jewish Civilization.) New York: Oxford University Press. 1984. Pp. 255. \$24.95.

It is easy to regard this book as one-sided or old-fashioned. Social history is almost entirely missing from its pages; there are no tables, few statistics, and little consideration of impersonal forces or class divisions. Personalities stand at the center of this popular narrative of German Jewish history; they are its agents, they reflect its problems and aspirations, they lend it significance. Such an approach stands in danger of degenerating into a loosely woven web of biographies or, worse, into a collective portrait of "heroes." H. I. Bach's *The German Jew* comes close to that fatal abyss but is saved largely by the restraining force of a pervasive thesis that, in a book mainly for the general reader, is original and will interest even the specialist.

Medieval German Jewry, like Jewry everywhere before modern times, sought redemption from within its Jewishness by scrupulous adherence to divine commandment and by hope in supernatural deliverance. Political emancipation and cultural in-

tegration, following in the wake of the French Revolution, transmuted this traditional messianism into a modern variety focused not on the distant future and God's miraculous intervention in history but on the immediate prospects offered the Jews by modern society. Contemporary Western values, especially those of the Enlightenment, became the messianic force that would redeem the Jews, as well as the rest of humanity, from the exclusivity and superstition of the past. Thus, German Jews came to transfer their religious hope from their own tradition to the world outside, leaving their Jewish identity an empty shell. They began to be mightily concerned with "how they were doing" in joining up with the messianic process. Increasingly, they sought external recognition and approval with a passion that helps explain their phenomenal creativity in the arts and sciences.

The resurgence of anti-Semitism on a massive scale, beginning in the late nineteenth century, created a severe crisis of faith. The substitute faith in Western civilization had proved as deceptive as the pseudomessianic Sabbatian movement of two centuries earlier. And, even apart from its burden of anti-Semitism, European culture by the twentieth century had become so fragmented into specialized disciplines, so replete with trend and countertrend, that it could no longer serve as a beacon. Although most German Jews resisted the acceptance of disappointment, some turned to Zionism and others embarked on a spiritual renewal that achieved a new synthesis of Jewish religious tradition with Western culture. This synthesis was represented by four outstanding thinkers and community leaders of the early twentieth century: Hermann Cohen, Leo Baeck, Franz Rosenzweig, and Martin Buber.

Although subject to criticism in its details, Bach's dialectical scheme is a helpful tool for interpreting at least the intellectual aspect of German Jewish history. The author, a recently deceased amateur historian whose native tongue was not English, also wrote very well. Regrettable, however, are the abundant errors of fact, which should have been caught in the editing: names and dates are distressingly unreliable. Also unfortunate are the lack of either notes or bibliography and an occasional tendency to romanticize. And, to be sure, the imbalance created by focusing on individuals does prevent a properly weighted evaluation of modern German Jewish history in its totality. Yet as the elaboration of significant intellectual and religious shifts the book does make its contribution.

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ARTHUR PRINZ. *Juden im deutschen Wirtschaftsleben: Soziale und wirtschaftliche Struktur im Wandel,*

1850–1914. Edited by AVRAHAM BARKAI. (Schriftenreihe Wissenschaftlicher Abhandlungen des Leo Baeck Instituts, number 43.) Tübingen: J. C. B. Mohr. 1984. Pp. xii, 202.

This is a somewhat inconsistent work. Arthur Prinz, who taught economics at the *Berliner Freie Volkshochschule* until his dismissal in 1933 and at Dickinson College in the postwar years, was commissioned to write this book in the 1960s; he died in 1981 before completing the last chapter. He drew on studies predating 1970 and made little use of more recent scholarship, which has opened up new vistas in the economic and social history of the German Jews. As a member of the older generation of historians, he had a greater interest in the achievements of the Jewish banking and entrepreneurial elite than in the lives of retail shopkeepers and cattle and grain dealers. This emphasis is reversed in the last chapter, written by Avraham Barkai, who contends that the *Grossbürgertum*, many of whose members had dissociated themselves from Judaism through baptism and intermarriage, was not representative of Jews as a whole and that an emphasis on the successful achievements conceals the economic crisis of the Jewish middle class that started long before the Weimar era.

Barkai corrects Prinz's overestimation of the influence of Jewish private bankers on the industrialization of Germany. He concludes that Jewish entrepreneurs, with a few exceptions, played no notable role in the development and management of heavy industry in Germany. He demolishes the myth of the economic supremacy of the Jews in imperial Germany by showing how economically disadvantageous it was for them to persist in their traditional preference for self-employment and occupations in commerce in an era of mature industrial capitalism. His chapter, with its probing questions and richly suggestive speculations, is the most illuminating part of the book.

This criticism of Prinz should not detract from the value of his historical synthesis of the economic ascent of the German Jews and the unfavorable effects of the Great Depression on them. His survey shows the extent to which they remained a distinctive group in their social and occupational structure and their pattern of internal migration throughout the nineteenth century. It was expected that industrialization and urbanization, together with the elimination of legal discrimination, would end the "abnormality" of Jewish concentration in commerce and credit lending. This did not happen. The Jews continued to prefer self-employment and occupations in retail trade and textile manufacturing despite the fact that the biggest fortunes were amassed in the coal, iron, and steel industries. They took advantage of the opportunities offered by the ex-

pansion of Germany's economy in a distinctive group pattern of economic behavior, which was different from that of other Germans and was influenced more by their historic experiences than by the incentive of maximizing incomes and profits.

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JAMES F. HARRIS. *A Study in the Theory and Practice of German Liberalism: Eduard Lasker, 1829-1884*. Lanham, Md.: University Press of America. 1984. Pp. xvi, 174. Cloth \$22.50, paper \$10.75.

Eduard Lasker is a melancholy figure in nineteenth-century German liberalism. His "enduring victories on significant issues," as James F. Harris notes in this new study, "were few and are largely forgotten" (p. xiii). His death at fifty-four, in January 1884, came near the end of a trip to the United States undertaken within a "context of personal frustration and poor health" (p. 119). The parallels between Lasker's life and the rise and decline of the cause for which he struggled are evocative, but Harris's subject is Lasker the public man, one of the first of a new breed of professional politician.

Harris believes the sources are too sketchy and Lasker's idiosyncrasies too unexceptional to justify a traditional biography. Instead, he presents Lasker as a representative figure of German liberalism through his speeches, his essays, and the legislation he sponsored. Lasker thus emerges in these pages as an intellectual but disembodied force. Harris has worked through the relevant sources with care, and from them he distills the essentials of Lasker's outlook: faith in the *Rechtsstaat* "in which law rationally ordered and accurately reflected the needs of society" (p. 46), belief that progress could be achieved by legislative means, and commitment to the uniformity of constitutional practices. All of this, however, was coupled with an inadequate understanding of the commercial-industrial economy and the individual's place and power within it.

From this vantage point Harris interprets Lasker's and liberalism's failures primarily in intellectual terms; they could not cope with the problems posed by the *Interessenpolitik* that overtook Germany in the late 1870s. It is a familiar thesis, but one that is too one-dimensional, too much a throwback to the self-blaming analysis of liberal division and defeat as liberal weakness. Harris acknowledges the institutional and structural burdens—universal suffrage and rapid socioeconomic transformation—with which German liberalism had to contend, but he underplays their destructive effects on all attempts to devise doctrinal formulas that could mediate the clash of interests. Moreover, owing to his focus on the national parliamentary forum he does not at-

tend sufficiently to the centrifugal pull of regional and local attachments, of secondary consequence to a Berlin-based legislator like Lasker.

As for Lasker himself, one is left wondering to what extent his troubles were a matter of *Seele* as well as *Geist*. In his brief section on Lasker's personality Harris offers some shrewd insights on his subject's strengths and limits. His analysis would profit by taking them further. Political leadership depends on character as well as ideas, and Lasker was not the force in the late 1870s and early 1880s that he had been earlier. But Harris's treatment of the alignments and shifts within liberalism is too static and ideologically oriented for him to explore, for example, why Lasker became less important than new organizational talents like Eugen Richter and Heinrich Rickert. Seeking the whole man may have necessitated riskier and more tentative conclusions, but it would have come closer to defining Lasker's significance in its full contours.

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HANS-WERNER HAHN. *Geschichte des Deutschen Zollvereins*. (Kleine Vandenhoeck-Reihe, number 1502.) Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 214. DM 20.80.

Because it was associated with commerce, the German Zollverein was held for a long time in rather low esteem in Germany and was perceived at best as a handmaiden of Prussia's mission to unify Germany. Twentieth-century reappraisal of German history has resulted in a critical rewriting of Prussia's role in the founding and development of the Zollverein, as well as in a greater appreciation of the problems inherent in economic unification brought to the foreground by the experience of the postwar European Economic Community. As a result, the Zollverein has finally gained the recognition it deserves. Hans-Werner Hahn's work, published on the occasion of the 150th anniversary of the founding of the Zollverein in 1834, embodies these advances in scholarship and is also the first attempt in German to cover its entire history since Wilhelm Weber's *Geschichte des Zollvereins* (1867).

Hahn's monograph, which provides a most welcome factual and interpretative account of the Zollverein, is not a definitive work but is certainly more than a sketch, as the author modestly styles it. It not only furnishes a detailed and running narrative of the Zollverein's complex history but also makes a particular contribution by evaluating the events and putting them into the larger setting. For instance, one passage characterizes the initial customs policy in Berlin as a "Prussian policy in Ger-

many, rather than a Prussian policy for Germany" (p. 57). Hahn's cautious and competent appraisal of the impact of the new institution in the 1830s and 1840s should also be noted, as well as his explanation of what made the Zollverein really function: the ability of the Prussian bureaucracy to develop a spirit of teamwork among the customs services of the participating states.

Hahn's interpretations offer much that cannot be found in William O. Henderson's *The Zollverein* (1939), which has been received as the standard work on the subject. Henderson's in-depth research, however, is not superseded but only supplemented, because Hahn relies primarily on the monographic literature and to a much lesser degree on his own earlier, specialized investigations. As a result, Hahn's presentation often appears to be less solid. There are occasional lapses into the pro-Prussian rhetoric of earlier historiography (p. 47, for example). There are also factual errors: William I was not Prussian crown prince before his accession to the throne, and as king he met with Napoleon III in October, not November, 1861; moreover, the *Zollparlament* was opened in 1868, not 1867.

Also, the Zollverein's unique constitutional contribution to the Germany variety of federalism could have been emphasized more, since it served as a pilot for a system of constitutional weights and balances that allowed the member states to retain control of their administrative structures, while assigning overall legislative functions and policy decisions to the national government.

The work has a chronology, a bibliography, and an index. Maps would have been helpful, since historical atlases are not accurate (Waldeck joined the Zollverein in 1832, not 1838) and often lack essential details in their presentation.

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HERMANN-JOSEF RUPIEPER. *Arbeiter und Angestellte im Zeitalter der Industrialisierung: Eine sozialgeschichtliche Studie am Beispiel der Maschinenfabriken Augsburg und Nürnberg (M.A.N.), 1837-1914*. New York: Campus. 1982. Pp. 311. DM 58.

This book is part of a series that started in the 1970s and has significantly improved our knowledge of the process of class formation, in general, and of the structure, condition, and development of the German working class during industrialization, in particular. Hermann-Josef Rupieper embarks on a description of the demographic, local, and regional determinants of production and labor recruitment of a machine-building firm (the MAN) that was to become one of the most important enterprises in its

field before the First World War. The study is thoroughly researched and makes much use of the plentiful material preserved in the company archive and in the regional and the Bavarian state archives. The author carefully considers his results in the light of recent research on white-collar employment, the movement of workers and their behavior during labor disputes, and other topics, all of which he treats from a comparative standpoint.

The study deserves attention not least because it examines the history of the two originally autonomous plants that evolved into the MAN. The first of these (*Cramer-Klettische Maschinenfabrik*) was founded in Nuremberg in 1837; it became one of the major producers of railway wagons and bridges and was thus heavily dependent on the business cycle in railway building. The second (*C. Reichenbach'sche Maschinenfabrik*) was established in Augsburg, where there was a large demand for textile machinery in 1840. This company made a range of machinery, including steam engines, for the paper-making, printing, and other industries. In 1898, after a period of intermittently bad market performance, the two companies merged, and the MAN was on the way to becoming an important producer of gas and diesel engines.

Throughout this study Rupieper combines considerations of the different environmental influences that contributed to the formation of the working class in the machine-building industry in Nuremberg and Augsburg. One of his major findings is that employers in Augsburg controlled the labor market much more efficiently than their counterparts in Nuremberg did. Originally, the industrial production structure in Augsburg relied heavily on textile industries (and, in fact, the MAN initiated a change of that structure toward more diversified labor markets), whereas in Nuremberg a range of machine-building firms existed from the very beginning, so that labor moved frequently and wages were higher. Surprisingly, the Augsburg plant of the MAN developed much more efficient measures of implementing social policies at the plant level—measures that were combined with a distinctly authoritarian attitude on the part of the employers. In Nuremberg, by contrast, a sort of company council developed early, which mediated labor grievances without union interference. Yet in Nuremberg the unions were comparatively strong, while in Augsburg employers managed to control the labor force by "yellow" unions.

Thus, a high level of employers' welfare policy contrasted with low union influence, and vice versa. This also holds true for other employers such as Stumm and Krupp. Additionally, Rupieper shows that in Bavaria (as in the remaining parts of southern Germany) conditions differed from those in Prussia; for example, community labor offices

(*Arbeitsnachweise*) designed to regulate the labor market were strongly and successfully favored by the Bavarian government.

All in all, the study is very detailed and, indeed, sometimes too detailed, such as in the treatment of associational benefit regulations. But it may serve as an example of the important achievements reached by German social history in recent years.

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GERHARD HECKER. *Walther Rathenau und sein Verhältnis zu Militär und Krieg*. (Wehrwissenschaftliche Forschungen, Militärgeschichtliche Studien, number 30.) Boppard am Rhein: Harald Boldt. 1983. Pp. xii, 542. DM 74.

This book by Gerhard Hecker is a valuable contribution to the mounting literature on Walther Rathenau, the enigmatic German-Jewish industrialist, philosopher, and statesman. Rathenau's career as a philosopher, an influential *persona grata* on the inner periphery of power with access to imperial Germany's highest civilian and military leaders, and reconstruction minister and foreign minister in the early Weimar Republic would scarcely have been conceivable had he not been head of the *Allgemeine Elektrizitäts-Gesellschaft* (AEG). Historians, however, have found it difficult to combine Rathenau the businessman with his other roles. At the most profound level this undoubtedly is because Rathenau himself was ambivalent. A more prosaic reason, however, is that the sources make it extremely difficult to capture him on a day-to-day basis. Rathenau did not leave an extensive *Nachlass* of daily correspondence and business papers, and the relevant AEG files appear to have been lost. Possibilities for research are certainly improving, thanks to the complete edition of Rathenau's writings being edited by Ernst Schulin and his colleagues at Freiburg, a research project of which Hecker's book is an integral part. Still, tracing Rathenau's relationship to the German military establishment and to the problems of war is no easy task if one wishes to go beyond the mere rendition of his thoughts on these matters and see them from the perspective of his personal and business interests. Hecker has been exceptionally successful at this task. On the one hand, he has ransacked the archives and turned up important new material, not only on Rathenau but also on the AEG's role as a producer of military goods. On the other hand, he has put his story together very well and provided a more convincing and nuanced presentation of Rathenau's relationship to German militarism and imperialism than has been heretofore available.

Hecker provides some interesting new material on Rathenau's Prussophilia and the difficulties he had as a Jew in trying to gain full acceptance into Prussia's leading institutions, including the army. The author's main contribution, however, is to present a convincing and consistent portrait of Rathenau as an ambitious businessman whose self-interest was reasonably enlightened and who never was so much the child of his age that he could not transcend the nonsense about him to the point of realizing that Germany's prewar and wartime policies were driving the nation to disaster. Thus, although profiting and even profiteering from naval building and armaments, Rathenau ultimately viewed the prewar naval race and the military buildup of 1912–13 as dangerous to German security and financial stability. He supported the forced requisitioning of Belgian labor in 1916–17, but not the manner in which it was carried out, and he defended high war profits as an essential hedge against the costs of postwar industrial reconversion. Yet he opposed unrestricted submarine warfare because he was convinced it would not work, and he supported a negotiated peace. Rathenau seems to have shared Bethmann-Hollweg's delusion that Ludendorff could be used to "sell" a negotiated peace, and Rathenau maintained surprisingly good relations with Ludendorff until the end of the war, although he unfortunately failed to exercise the kind of influence enjoyed by his fellow industrialists Hugo Stinnes and Carl Duisberg. As Hecker shows, Rathenau's call for a "levé-en-masse" in October 1918, however much influenced by his admiration for the Prussia of 1813, had the practical goal of trying to remedy Ludendorff's precipitous demand for an armistice, which, as Rathenau correctly recognized, was militarily unnecessary and placed Germany completely at the mercy of the Allies.

Perhaps the most interesting of Hecker's findings pertains to Rathenau's decision to resign as head of the raw materials section of the Prussian War Ministry, for whose creation in April 1915 he had been largely responsible. Although other historians, including this reviewer, have presented a variety of explanations for Rathenau's decision, Hecker convincingly pieces together old and new evidence to argue that Rathenau left in anger over Karl Helfferich's appointment as state secretary of the treasury. Rathenau apparently wanted the post and did not wish to be forced to go constantly to his successful competitor with requests for money. Whether he would have done a better job is hard to say, but at least his failure made it impossible to blame a Jew for the fatal mismanagement of Germany's wartime finances. Instead, Rathenau was to be murdered for his desperate efforts to cope with the legacy of

Helfferich and the military leadership he had so fervently, but critically, admired.

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JAMES F. WILLIS. *Prologue to Nuremberg: The Politics and Diplomacy of Punishing War Criminals of the First World War*. (Contributions in Legal Studies, number 20.) Westport, Conn.: Greenwood. 1982. Pp. xiii, 292. \$29.95.

James F. Willis views the unsuccessful attempt of the Allies to bring to trial those leaders of the Central Powers whom they thought to be war criminals as "the principal precedent for Nuremberg" (p. xi). Although one has no disagreement with Willis's professed sympathy "with the idea of international war crimes trials" (p. xii), one does expect at least an attempt to define what in the course of World War I, internationally or from the perspective of the Allies, was considered a war crime. An explanation seems fundamental to a more thorough analysis and understanding of the demands for extradition and trial and even of the German refusal to cooperate.

On the basis of considerable research in European and American archives, Willis reports the evolution of the Allies' sentiment for trials from early in the war to the postwar period, which was characterized by lack of unity and purpose. The invasion of Belgium, the conduct of German troops in that country, the air raids on Britain, the unrestricted deployment of submarines, the outrages against the Armenians, and individual cases, such as the shooting of the British nurse Edith Cavell or the execution of the British ferry captain Charles Fryatt—these all make up the register of charges against the Germans and their allies. But, if German war measures such as unrestricted submarine warfare are discussed, one might ask whether attention ought not to be given also to the British blockade, in which no distinction was made between military supplies and food for civilians. One may also wonder whether the use of submarine traps, known as Q-ships, and events such as the *Baralong* incident and the harsh British reprisals following the 1916 Easter Rising in Ireland do not deserve more weight in the final score of the game of brutality—if, indeed, such a score for World War I, possibly in contrast to World War II, should be thought useful.

One is not surprised to find that the list of persons originally picked to be tried is in sharp contrast to the handful of unimpressive underlings who finally in 1921 made their appearance in Leipzig before what could hardly be described as an impartial international court. The requests for extradition or trial of the well known were eventually dropped for

political reasons. Significantly, all proceedings came to an end with the National Socialist takeover even though other legal negotiations, such as the Mixed Claims Commission, were continued by Hitler's representatives, a curious difference not mentioned by the author.

Willis's indirect suggestion that the failure to hold war crimes trials after World War I and to reach agreements concerning international jurisdiction contributed "to the barbarous character of the Second World War" (p. 173) appears somewhat unrealistic. Even the author's theses that the tribunals following World War II did establish "firm legal precedents in international law" and that, "henceforth, all men were to be legally obliged not to use force in the service of their nation in war except in compliance with the requirements of international law" (p. 175) seem to lack relevance in view of the brutal realities of more recent wars.

Regrettably, the misspelling of German names mars an otherwise readable text.

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WOLFGANG JÄGER. *Historische Forschung und politische Kultur in Deutschland: Die Debatte 1914–1980 über den Ausbruch des Ersten Weltkrieges*. (Kritische Studien zur Geschichtswissenschaft, number 61.) Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 322. DM 68.

Wolfgang Jäger's book is a valuable contribution to the study of the political context and function of historical scholarship in twentieth-century Germany. Jäger is right to choose the debate on the outbreak of World War I as the theme around which the conflict of historical and political values can best be organized. Two excellent studies on history and political ideologies in the Weimar Republic have appeared: *Ideologie des deutschen Weges* (1980) by Berndt Faulenbach in the Federal Republic of Germany and *Die bürgerliche deutsche Geschichtsschreibung der Weimarer Republik* (1975) by Hans Schleier in the German Democratic Republic. Like Faulenbach and Schleier, Jäger proceeds from the assumption that historical scholarship can be understood only in the context of factors external to the discipline, which Jäger, using the terminology of the "critical theorists," identifies with "erkenntnisleitende Interessen," which reflect the historian's "political and moral convictions" (p. 10).

The history of the debate can easily be divided into two periods that characterize two very different phases of German historiography. There is a remarkable continuity in German historical writing and scholarship through the four different political orders from Wilhelminian Germany to the Federal Republic in the early 1960s. Jäger demonstrates how

the conservative national consensus, which asserted German innocence and reaffirmed the basic soundness of the state created by Bismarck, survived the Nazi horror. Gerhard Ritter and Hans Rothfels had the audacity to proclaim that Nazism was an outgrowth of Western democracy with no roots in the German-Prussian traditions. Jäger shows how far to the Left the consensus on Germany's relative freedom from responsibility for World War I went, including not only critics of the Bismarckian state such as Johannes Ziekursch but also pacifists like Ludwig Quidde and Walther Schücking.

The second period begins with the Fischer controversy. Jäger rightly gives Fritz Fischer credit for two achievements. Using relatively traditional methods of documentary study, Fischer raised serious questions about German policy in the summer of 1914 and about German war aims—questions that opened a reexamination of German responsibility for the First World War. The second great achievement was the impulse Fischer gave to the new attempts to study political history in its social, economic, and ideological context. The questions Fischer raised regarding the sources of German policy could not be answered by traditional methods of political history. The new critical social history, as Jäger shows, had its origins among the dissidents in the Weimar Republic, particularly Eckart Kehr, but it was only in the wake of the Fischer controversy that this perspective gained serious consideration in West Germany. Jäger follows the discussion on the outbreak of the First World War into the 1970s, discussing not only the new theoretical and methodological approaches of the advocates of a "historical social science," such as Hans-Ulrich Wehler and Jürgen Kocka, but also those of the less well known, social science-oriented peace researchers and of the defenders of a more traditional approach to international politics, such as Andreas Hillgruber and Klaus Hildebrand, and of a historian such as Wolfgang Mommsen, who in Jäger's opinion represented an intermediate position. A final section deals with studies in the German Democratic Republic and examines the basic differences that separate these from the liberal-democratic position of the critical orientation in the Federal Republic.

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JOHN H. ZAMMITO. *The Great Debate: "Bolshevism" and the Literary Left in Germany, 1917–1930*. (American University Studies, ninth series, History, number 4.) New York: Peter Lang. 1984. Pp. 208. \$25.00.

This book, though small, does at least three things. First, it discusses the various meanings and uses of

the term "bolshevism" in Weimar Germany. Second, it traces the evolution of the German literary Left's relation to the Soviet Union in the 1920s. Last, it describes the debates within Weimar's leftist community concerning the nature of artistic commitment and the role of the intellectual in political life. In a brisk and economical style, John H. Zammito makes a number of penetrating observations about German literary politics in the 1920s. To do this, however, he is forced to assume much prior knowledge about Weimar on the part of the reader. For those well acquainted with the material, new insights and connections abound, making this study most rewarding.

The first theme is perhaps the most interesting. As Zammito demonstrates, the term "bolshevism" meant everything and nothing. The far Right used the term to slur everything left of Center. The German Social Democratic party used it to discredit those on the Left who were pro-Russian. The left-wing of the Independent German Social Democratic party adopted bolshevism as a positive concept, but with disastrous results since the party aspired to repeat the Soviet experience on German soil. The German press wrongly equated bolshevism with the council movement, which in fact represented an alternative to Bolshevik centralism. For still others bolshevism was "cultural bolshevism," that is, modernist experimentation in the arts. By explaining this term's many uses, Zammito opens up a whole intellectual and cultural landscape. Unfortunately, the analysis of bolshevism slips away in the latter half of the book, but not before Zammito has helped clarify the extraordinary power of images and illusions in Weimar Germany.

In dealing with the second theme, Zammito sees three phases in the Left's relationship to the Soviet Union. From 1917 to 1920 groups like Berlin Dada showed unqualified enthusiasm for the Russian Revolution and for bolshevism, even though they had little understanding of what was going on in Russia. From 1920 to 1923 there was a switch to the "cult of the proletariat" in the wake of imported Soviet ideas. And from 1923 to 1930 the gradual but eventually almost total bolshevization of party-oriented German literary theory occurred, thanks to the Comintern's demand for intellectual uniformity. By 1930 the German far Left had embraced a concept of literature that was wholly Leninist (as interpreted by Stalin). Zammito tells this tragic but fascinating story accurately and well.

Zammito's third theme focuses on the literary debates, largely conducted in the extreme left-wing journals, concerning the role of the writer or intellectual in politics. Each of the issues discussed evoked the most passionate yet divergent responses from Weimar leftist writers. Should an author remain an autonomous thinker, or should he defend

Parteilichkeit? Should he take a position "with" the proletariat or merely "along side" it? Is it still important to "know" the world better, or must the primary stress be placed on changing it? These debates were intense and urgent and involved some of the leading leftist writers and critics of the time: Bertolt Brecht, Alfred Döblin, Johannes R. Becher, Siegfried Kracauer, Bela Belasz, Walter Ihering, Georg Lukács, Walter Benjamin, and many others. In the last part of the book Zammito gives special attention to the positions of Brecht, Döblin, and Benjamin. Here perhaps more than anywhere else in the book he seems to be covering familiar ground, but he does so with fresh perspectives.

The book is marred by a number of misspellings. It also lacks a bibliography and index. But Zammito does illuminate important aspects of German literary politics in the 1920s that have not been much discussed in English, and this makes the book well worth recommending.

DAVID GROSS
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RENATE BRIDENTHAL *et al.*, editors. *When Biology Became Destiny: Women in Weimar and Nazi Germany*. (New Feminist Library.) New York: Monthly Review. 1984. Pp. xiv, 364. Cloth \$27.50, paper \$12.00.

This useful and timely study brings together fourteen articles by a variety of authors. Most have been published before, but many have been revised for this collection, some appear in English for the first time, and a few are entirely new. They show the disastrous consequences for women's freedom and equality of the antifeminist policies adopted by the Nazis and already in operation to some extent in the Weimar Republic. The authors illustrate the "dangers implicit in a feminism that celebrates separate spheres and differences between the sexes, glorifies motherhood and women's bodies" (p. xi), and so easily falls prey to the lure of ideologies detrimental to women's rights and, indeed, human rights in general.

The book is thus more than a simple collection of essays: despite some differences of emphasis and interpretation (for example, over the definition of feminism), the contributions come together to demonstrate by means of a historical parallel the implications of the antifeminist policies of the Moral Majority in the U.S., the "new prudery" in West Germany, and the conservative advocates of a return to "Victorian values" in the United Kingdom. One drawback of this approach is that it leads to a tendency to portray women as victims, culminating in the last two essays in the book, which are personal reminiscences of women who suffered under the

Third Reich. In emphasizing the dangers of anti-feminist policies, the authors ultimately fail to propose any fully worked-out alternative or to suggest how these policies might be resisted. In the end, therefore, this is a pessimistic book. With this reservation, however, it is a fine example of the contribution that committed scholarship can make to historical research.

A major achievement of the book is to present for the first time a series of studies of conservative and right-wing women's organizations, from the housewives' associations to the "Nazi feminists" of various hues whose activities were so quickly stopped after the "seizure of power" in 1933. The authors do, it is true, underestimate the radicalism of mainstream German feminism before 1914 under the influence of the "radical feminists" (self-styled, not labeled so, as wrongly claimed on page 204) whose views were dominant from 1896 to 1908. But several essays—above all, a fascinating account of the "Bremen morality scandal" of the 1920s—redress the balance for the Weimar period by showing that there were progressive feminists about. A welcome feature is that the articles are for the most part unpolemical, despite the odd reference to the sins of an anonymous "male history" that turns out on closer inspection to have been at least partly written by women (pp. 288, 290 n. 10).

The weaknesses of a "gender analysis" of historical problems come to the fore in more than one essay. The claim that "the explanations for women's persistent loyalty to conservative parties can be found in the reactions of politicians to their new women constituents" (p. 36) neglects the influence of religion on female voting choices and ignores the fact that women voters deserted the conservatives and liberals for the Nazis for reasons that had little to do with their policies toward women, which were more or less the same in all political parties to the right of the SPD. The book's claim to revise the view that German feminism had affinities with liberalism (p. xiii) is unconvincing because it ignores the complex, changing, and, in some ways, peculiar character of German liberalism between 1890 and 1933. Particularly objectionable is the essay on "Women and the Holocaust": on the whole, women were victims of Nazi genocide and persecution not because they were women but because they belonged to other reference groups, and the author does the male victims of Nazism a serious injustice by ignoring their sufferings. In general, however, this is an exciting and timely book, well written, carefully researched, and intelligently argued: women's history at its stimulating and controversial best.

RICHARD J. EVANS
University of East Anglia

JACQUES R. PAUWELS. *Women, Nazis, and Universities: Female University Students in the Third Reich, 1933–1945*. (Contributions in Women's Studies, number 50.) Westport, Conn.: Greenwood. 1984. Pp. xv, 206. \$29.95.

Despite the misogyny of the National Socialists, women were more successful in gaining admission to German universities in the Third Reich than at any other time. Initially, the Nazis did their utmost to discourage women from studying by using actual restrictions and specious eugenic arguments. They put out such nonsense as the claim that 80 percent of female graduates were permanently sterile and that long hours of sitting in lectures led to menstrual disturbances!

The overall drop in university enrollments in the 1930s has often been blamed on the anti-intellectualism of the Nazi leadership. Jacques R. Pauwels demonstrates that the decline was actually caused by demographic factors: the small birth cohorts of the First World War simply produced fewer eligible matriculants. Although this observation was made by contemporary pundits like Edward Hartshorne and has recently been repeated by Bernd Zymek, Pauwels reinforces the point through a detailed and methodical account of the "intrinsically ineffective or irrelevant (though ostensibly energetic) measures" (p. 136) through which the Nazis sought to restrict women's higher education. From the start the regime ignored its own deterrents, such as quotas, and women's enrollments declined less steeply than men's.

Nazi education was meant to be not just academic but also political. Yet the female student leadership appointed to this latter task faced continual problems. The Working Community of National Socialist Women Students (ANSt) was a minor organization that left few records. Pauwels has mined them exhaustively to produce in a few chapters a definitive study of the ANSt, which corroborates, with many colorful examples from the educational sector, established perceptions of the misogynous nature of Nazi society and the deleterious effects of "institutional Darwinism." The ANSt leadership was constantly sparring with the male student organization (NSDStB) and the other Nazi women's groups. The NSDStB's behavior was insulting in the extreme: it excluded women from the departmental groups that discussed the politicization of scholarship and fobbed them off with cooking classes instead. Even to enter the national prize essay contest, women students had to demonstrate proficiency in household chores! What Pauwels fails to stress sufficiently is that the NSDStB, although more powerful, was scarcely more successful in its own ideological endeavors than the ANSt.

The author presents his argument vigorously but sometimes exaggerates. To call Bernhard Rust, the ineffectual minister of education, "an indulgent protector of Germany's women students" who was "forced" by shortages in the professions to encourage his "vassals" in the student leadership to act leniently toward women is arguable on several counts (pp. 140–41). Even though women received proportionately more scholarships during the war, the regime was not "virtually humiliating itself . . . in a shameless effort to bribe" them into taking up university studies (pp. 98, 140). And there is no persuasive evidence that the majority of women students looked down on Hitler as "a philistine little Austrian corporal and a despicable upstart" (p. 91). Pauwel's central argument is convincing without such statements. The presence and activity of women at universities in the Third Reich was only coincidentally affected by official Nazi policy.

GEOFFREY J. GILES
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Documents on German Foreign Policy, 1918–1945. Volume 6, *November 1, 1936–November 14, 1937*. (The Third Reich: First Phase, series C, 1933–1937.) Washington: Government Printing Office. 1983. Pp. lxxxii, 1140.

After World War II the United States and Great Britain, subsequently joined by France, began a project for publishing German diplomatic documents. Originally intended to cover the years 1918–45, the program was narrowed to the years 1933–41, with the volumes to be issued in two series, C and D. The Federal Republic of Germany, which eventually joined the project and took over the original documents, then continued the program, completing series B for 1925–32 and E for 1942–45, with A for 1918–25 now in the process of publication. The Germans have also taken the lead in publishing the German-language edition of the later volumes of series D and all of series C. The last volume of the English-language edition of series C is under review here. The first appeared in 1957; four more came out by 1966; and the final volume now follows eighteen years later! This means that scholarship on the period covered, November 1936 to November 1937, has long since dealt with most of the issues and has cited a high proportion of the documents published. Thus, this reviewer's two-volume *Foreign Policy of Hitler's Germany* (1970; 1980) refers to the documents by the numbers assigned to them in this volume. There, too, the reader can sometimes find references to the location in the Potsdam archive of documents referred to as not found by the editors.

If the outrageous delay in the appearance of this volume robs it of much of the novelty of others in this massive project, the completion of the series is welcome all the same. Like its predecessors, this book will be of great value to students looking for an entrance into the problems of the time. Like its predecessors, it certainly looks to this reader like a highly competent and objective selection from the archives; whatever criticisms might be made, the editing is free of tendentiousness. Unfortunately, also like its predecessors, the volume leaves it up to the reader to discover, sometimes by laborious checking of the serial numbers printed above each document, the archival provenance of every individual document—a defect that might easily have been remedied (as it is in the collection of French diplomatic documents) and that was called to the attention of the editors over thirty years ago.

Some major themes illustrated in this volume deserve mention. There is extensive documentation on the abortive negotiations for a new Western pact to replace the Locarno agreements; on German interests in East Asia, especially the projects of Hans Klein in China and the mess Hermann von Raumer made of the anti-Comintern pact negotiations; the German efforts to direct Hungarian revisionist aspirations away from Yugoslavia and Romania and instead toward a war against Czechoslovakia (nos. 53, 98); the attempts to draw Romania into the German orbit and to neutralize Belgium; the temporary rapprochement between Italy and Yugoslavia; Germany's troubles with the Vatican over National Socialist attacks on church schools; the problems in German-Polish relations, including the expiring minorities treaty for Upper Silesia and the role of Carl Burckhardt as new high commissioner in Danzig; the continued estrangement of Germany and the United States, in which arguments over trade with Brazil were an important irritant; and the interests of the British and French governments in a general settlement with Germany, an interest never reciprocated by Berlin but continuing in the imagination of some scholars.

Readers will be interested in the complete English texts of documents previously printed in German by J. W. Brügel in *Stalin und Hitler* (1973; nos. 7, 8), on Stalin's attempt to attain a rapprochement with Germany through David Kandelaki, the head of the Soviet trade delegation in Germany (nos. 183, 195), the continued availability in Germany of foreign exchange for such projects as the financing of Konrad Henlein's Sudeten German party (no. 204), the attempts of the German Foreign Ministry to keep up with the economic negotiations conducted by Gauleiter Erich Koch with Yugoslavia, and the information on documents stolen from the British embassy in Rome (nos. 484, 487). They will be intrigued to learn that yet another of the spectacular

documents once denounced as forgeries (like the Hossbach memorandum, the letter of General von Fritsch of December 11, 1938, and the report on Hitler's speech of August 22, 1939) turns out to be authentic after all; in this case it is the so-called Patagonia Document on German interest in the southern portion of Argentina (no. 137).

Since the last volume of series C chronologically overlaps the first volumes of series D, its publication is especially important. All those who work on the history of the interwar years will wish to check this volume for information on their topic and will hope that the editorial group in Bonn, aided by British and French experts, will be able to complete series A soon. Anyone wanting some insight into the reasons for the endless delays in an enterprise whose first volume appeared in 1949 will wish to read "The Quadripartite Project *Akten zur deutschen auswärtigen Politik 1918–1945: Experiment in International Historiography*" (in *Russland—Deutschland—Amerika: Festschrift für Fritz T. Epstein* [1978]), a memoir and analysis by Hans W. Gatzke, the American scholar who deserves far more credit for his role in this project than most have accorded him.

GERHARD L. WEINBERG
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GERD R. UEBERSCHÄR and WOLFRAM WETTE, editors.
"Unternehmen Barbarossa": *Der deutsche Überfall auf die Sowjetunion, 1941; Berichte, Analysen, Dokumente.* (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1984. Pp. 416. DM 48.

This anthology is best evaluated as a complement to the controversial fourth volume of *Das Deutsche Reich und der Zweite Weltkrieg* (1983), prepared by the Federal Republic's *Militärgeschichtliches Forschungssamt* (MGFA). Five of the twelve contributors are affiliated with the MGFA. Most of the others are commonly identified with an emerging *Forschungssamt* position on the origins and nature of World War II in general and the Russo-German War in particular. Its key is the assertion that Nazi Germany's best efforts were doomed from the beginning because of hopeless strategic and economic inferiority. More specifically, it claims that Operation Barbarossa was not a desperate response to a Russian threat but rather an ultimate gesture of ideologically and racially motivated aggression. Far from being unique to Hitler, this arrogant mind-set dominated the Wehrmacht and permeated the civilian elites of Nazi Germany. It survives, albeit in an attenuated form, in those West German political and intellectual circles that offer apologies for Hitler's defeat or stress the impossibility of permanent rapprochement with communist Russia.

The first section of the book includes four essays on media images. Gerhard Schreiber insists that between 1951 and 1981 serious scholars and responsible journalists increasingly have agreed that Barbarossa began neither a preventive war nor a conflict with traditional objectives but a war of enslavement and annihilation. The interpretation, though defensible, suffers from overstatement. A similar tendentiousness characterizes the section's other contributions. Two endorse "Der unvergesene Krieg," a Russian series shown on West German television in 1981–82, on the basis that it increased sensitivity to the Soviet viewpoint on the war. But they inappropriately dismiss criticism of the broadcasts as neo-Nazi or national-conservative in inspiration. H.-H. Nolte is even less successful in equating the ethnocentrism of West German textbooks with the officially enforced orthodoxy of their Soviet counterparts.

Part 2 is intellectually solid but hardly a compendium of bloodless scholarship. Perhaps the best piece in the book is Andreas Hillgruber's revision of an essay, originally published in *German Studies Review* in 1979, on the relationship of Barbarossa to the Final Solution. He argues persuasively that Hitler issued verbal orders at the end of March 1941 to destroy all Jews in occupied Russia and then expanded their application to the rest of Eastern and Central Europe. This blends well with Gerd Ueberschär's interpretation of Hitler's war on Russia as waged from a combination of economic and ideological motives, little influenced by rational planning. In a second contribution Ueberschär intelligently summarizes the Blitzkrieg's failure, stressing the Wehrmacht's underrating of the USSR's military potential and the Red Army's fighting power. Wolfram Wetze's summary of the campaign's propaganda preparation combines with Lutz Lemhöfer's account of the churches' attitude to show the relative absence of popular enthusiasm for the Russian war. On the other hand, Rolf-Dieter Müller and Christian Streit reprise their earlier publications to demonstrate the culpability of Germany's military and economic establishments. Müller presents Barbarossa as an extended plundering expedition, relying on force and terror to the detriment of systematic exploitation of the occupied territories. Streit sharpens his argument that the Wehrmacht practiced ideologically inspired genocide against Soviet prisoners of war. Manfred Messerschmitt concludes the section by insisting that the Russo-German War is too often sentimentalized in West Germany as a crusade in defense of Western civilization. In such company it is hardly surprising that Arnold Sywottek presents Soviet war aims as reactive and defensive, based on a desire to free Russian soil from its bestial invaders and reflecting no sign of an

integrated program of extending Russian rule over other states.

One need hardly be an unreconstructed cold warrior to suggest that this pattern of interpretation, however pure the motives of its advocates, is bound to generate charges of misusing history for political purposes. It strongly reinforces lines of argument standard in the German Democratic Republic and the USSR that present Russia as innocent victim and ruthless avenger. Whether it is something more—a subtle appeal for German neutralism or an even more subtle attempt to prove that a Germany unable to defeat an emerging Soviet Union poses no threat at all to the developed Soviet superpower—remains debatable. But the MGFA bids fair to have inaugurated a worthy successor to the Fischer debate of the 1960s.

One hopes that the outcome of this controversy will be determined in libraries and archives rather than through the polemics that remain an academic art form in Germany. And here the last two sections of the present work are significant. Müller and Ueberschär present an outstanding critical bibliography of German- and English-language scholarship on Russo-German relations in the context of Barbarossa. Ueberschär supplements this with a hundred pages of well-indexed and unfamiliar documents. The tip of a massive research iceberg, they show that this work's line of argument must be refuted. It cannot be dismissed.

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BUNDESARCHIV UND INSTITUT FÜR ZEITGESCHICHTE.
Akten zur Vorgeschichte der Bundesrepublik Deutschland, 1945–1949. In five volumes. Munich: R. Oldenbourg. 1976–83. Pp. 1197; 654; 1062; 1076; 1151. DM 1,034 the set.

The progressive expiration of the thirty-year *Sperrfrist* governing access to materials in West German archives has spawned a growth industry in postwar German history. One welcome product of this has been the publication of several excellent documentary collections, including this five-volume joint project of the Bundesarchiv and the Munich-based Institut für Zeitgeschichte. A major purpose of the project is to establish the political roots of the Federal Republic and to determine the extent to which the Germans themselves, in spite of their nominal powerlessness, influenced decisions that helped shape the Federal Republic. This perspective, as opposed to one that simply treats the Germans as objects of the occupying powers, is long overdue. Yet, as Diethelm Prowe has pointed out in a thorough review of the first volume (*Central European History*, X/1 [1977]), the concern with political roots

and decisive turning points makes the series a more consciously positive historical statement than is usually the case with documentary collections, while the emphasis on administrative decision-making produces a restricted view of the range of alternatives that existed. To this it should be added that the decision to omit discussions of more mundane problems (*reine Kriegsfolgen*), though justifiable on grounds of space, reduces the usefulness of the collection for social historians interested in learning more about the war's immediate impact on German society.

Each volume includes a solid historical introduction and discussion of the source materials. The collection as a whole provides a useful core of documents, and the extensive notes, annotations, and indexes make it an invaluable guide to postwar materials in the Bundesarchiv and other West German archives.

The first volume covers the period from September 1945 to December 1946, beginning with the first attempts to coordinate governmental activities in the American and British zones and ending with the decision to merge the two zones into Bizonia. The documentary core of the volume consists of the protocols of the *Länderrat* of the American zone of occupation and, for the British zone, the protocols of the *Konferenzen der Chefs der Länder und Provinzen*, followed by those of the *Zonenbeirat*. Also included are minutes of the various meetings that took place between German officials of the two zones of occupation. In both zones administrators, not politicians, were initially the main German actors. Although eventually eclipsed by the formation of bizonal organs, the *Länderrat* and *Zonenbeirat* were important training grounds for future German leaders, and in many cases their actions significantly shaped the German political environment before the political parties emerged on the scene.

The second volume covers the first half of 1947, a period marked by an intensification of the cold war, deepening economic and foodstuff crises, and the initial failure of the bizonal apparatus. The documentary foundation of the volume remains largely the same as that of the first. Although the Moscow meeting of the Council of Foreign Ministers (March–April 1947) marked the emergence of German political life, as parties and political leaders began to take stands on Germany's future, it also worked to retard the development of Bizonia, since Anglo-American authorities were determined to avoid any moves that would give the appearance of trying to establish a separate German state. In the end the hope of a four-power resolution of the German problem, as well as the expectation that the new bizonal institutions would quickly solve the economic problems of the American and British zones, remained unfulfilled. The Munich Minister

Presidents Conference of June 1947 signaled the growing estrangement of the western and eastern halves of divided Germany and set the stage for new efforts by the great powers to organize more effectively the areas under their control.

The third volume covers the second half of 1947. Internationally, the period was framed by the collapse of the Moscow meeting of foreign ministers and the equally unsuccessful London meeting of December. Internally, it was framed by the successive reorganizations of bizonal institutions that followed the Moscow and London meetings. The increased significance of Bizonia in its second phase is reflected in the changed documentary base of the volume, which is formed primarily by the protocols of the executive committee of the Economic Council *Wirtschaftsrat*. Although conceived by the victors largely as an improved administrative device, the council soon began to function as a quasi-parliament. Yet, if, as the German phrase puts it, some "switches were set" for the future, the situation was still fluid.

The fourth volume covers 1948, the crucial and, in retrospect, decisive year of the occupation period. In 1948 occupation officials increasingly began to implement policies that made the formation of a West German state likely and, simultaneously, helped shape its political, social, and economic content. In the process the relationship of the victors to the vanquished changed noticeably. Meetings between Anglo-American military government leaders and German officials took place on a regular basis, and the former proved increasingly willing to share information with—and make concessions to—the latter. French military officials also began, belatedly, to coordinate activities within their zone and to prepare for its eventual merger with the other western zones. Meanwhile, bizonal institutions (again reorganized in early 1948) began to take on the characteristics of a government in embryo. These developments are mirrored in the selection of the volume's documents: the foundation is provided by the protocols of the *Direktoriatsitzungen*, which are supplemented by minutes of the meetings between military government officials and German leaders from the American and British zones; French activity is represented, if scantily, by reports of the meetings of General Koenig with German leaders in the French zone of occupation.

The accelerated pace of German self-government and the emergence of a form of shadow cabinet are traced in the final volume of the series, which covers the period from January to September 1949. Its documentary base is essentially the same as its predecessor's, with the protocols of the *Direktoriatsitzungen* forming the core. The protocols are excellently fleshed out through notes and insertions of additional materials that provide fuller context and

content. The decisions of 1949 led purposefully to the formation of a West German state and many of them had the character of *Vorentscheidungen*, decisions that irrevocably shaped the nature of West German society and politics. At the same time there were differences between German and Allied perceptions of how many and what kinds of decisions should be made by what were essentially administrative rather than parliamentary procedures. Fearful of long, drawn-out parliamentary struggles over controversial issues, German Bizonia officials tried to legislate as many issues as possible before the establishment of the new state, while military government officials countered those attempts that they considered unwisely preemptive through their veto powers. The result was that, while many crucial decisions that helped ease the transition from occupation to semi-sovereignty were made, a number of complex issues still remained on the agenda of the newly constituted Bundestag.

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GABRIELE STÜBER. *Der Kampf gegen den Hunger, 1945–1950: Die Ernährungslage in der britischen Zone Deutschlands, insbesondere in Schleswig-Holstein und Hamburg*. (Studien zur Wirtschafts- und Sozialgeschichte Schleswig-Holsteins, number 6.) Neumünster: Karl Wachholtz. 1984. Pp. xix, 935. DM 65.

One picks up a 935-page book with trepidation, but scholars will find much of value here. More than a history of the daily struggles of individual Germans in Schleswig-Holstein and Hamburg to find adequate nourishment, this is also a useful study of the development and policy of the British government of occupation. Data on food production and consumption from throughout the British zone and Berlin are regularly compared with data from prewar and postwar Germany. Forty-two tables, several administrative charts, 54 pages listing archival sources, and a 34-page bibliography will make this a useful reference for specialists.

Gabriele Stüber's argument is clear and straightforward. The Nazis learned from World War I and prepared for shortages by creating a ministry to regulate food production and ration its distribution. Hence, the Germans did not starve during the war, although toward the end the bombing, population movements, and allocation of resources to industry led to worsening conditions. The war's end was no zero hour in the area of food supply. The British kept Nazi administrative structures largely intact on the regional and zonal levels, although the absence of a nationwide central authority lessened the effectiveness of that structure. Quickly, however, a cata-

strophic situation developed. The loss of food-exporting areas east of the Oder-Neisse line (25 percent of the prewar food-producing area), the stream of refugees (the population of Schleswig-Holstein grew from 1,645,700 to 2,435,000 between February and June 1945, and there were also 1.4 million prisoners of war to feed), lower agricultural productivity (owed to shortages of fuel and fertilizers), and the disruption of market mechanisms (owed to divisions between the zones, lack of transportation, and the collapse of the currency)—these factors resulted in serious food shortages that lasted until the fall of 1948. In the summer of 1945 the legal daily food ration in some cities fell below 700 calories; in March 1946 in Schleswig-Holstein it was only 1080. Unable to master the situation alone, the British opted for a merger with the American zone. The food administration of Bizonia became entangled in jurisdictional conflicts, however, and was unable to bring any real improvement. The good harvest of 1948, the massive food imports paid for by the Marshall Plan, and the currency reform finally led to better nourishment and increased food production, although the latter reached 1938 levels only in 1949–50. Not until April 30, 1950, was food rationing ended.

Why did the Germans not starve after the war? Some of the food shortfall was made up by private charities: the Red Cross, the churches, the trade unions. Foreign aid was of key importance. The bulk of the aid came from the U.S., although in per capita terms the Swiss, Swedes, Chileans, and Brazilians contributed more. From 30 to 50 percent of the population regularly engaged in illegal activities to supplement its diet: foraging trips to the country, barter arrangements, black marketing, and theft. The food thus obtained did not give Germans a normal diet, but it sufficed to prevent long-term physiological damage.

The prolonged food crisis, Stüber argues, had more important political than physical consequences. The takeover of the Nazi administration and the powerlessness of government in general undermined efforts at democratization. Germans were politically apathetic and concentrated on daily survival. There was no direct confrontation with Germany's Nazi past or with the issue of guilt. Stüber does not blame the British. The responsibility for the situation lay with the Germans, and, as bad as the situation was after 1945, the inmates of the German concentration and prisoner-of-war camps had seen worse.

JEFFREY M. DIEFENDORF
University of New Hampshire

HANS GEORG LEHMANN. *Öffnung nach Osten: Die Ostreisen Helmut Schmidts und die Entstehung der Ost-*

und Entspannungspolitik. Bonn: Neue Gesellschaft. 1984. Pp. 256. DM 24.

Hans Georg Lehmann has written a useful book but one that in many respects must be considered "court history." Helmut Schmidt is clearly its hero and all others are not.

The book has two parts. The first is a detailed account of Schmidt's first two trips to the Soviet Union and Eastern Europe. The second is an analysis of the origins of Bonn's *Ostpolitik*. Lehmann's main thesis is that "Helmut Schmidt conceived and began the SPD's [Social Democratic party's] Eastern and detente policies. Brandt and Bahr realized them on his basic line, but with goals that went much farther and that failed" (p. 184).

Lehmann's two principal distinctions between Schmidt's and Brandt's *Ostpolitik* are that Schmidt stressed the importance of Bonn's NATO ties as the irreplaceable basis of the policy and that he wanted to uncouple the SPD's advocacy of detente with the East from the German question (that is, reunification). Lehmann's critical analysis of Brandt and Bahr seems to this reviewer basically correct: Bahr's policy was one of German nationalism, of absolute priority for reunification; Brandt wanted some kind of European solution. Both men tended toward euphoria, whereas Schmidt was more pragmatic. Indeed, one can see in Schmidt's constant emphasis on military parity in the 1960s the germ of his later insistence on a NATO reply to Soviet SS-20 deployment. In contrast, Brandt's and Bahr's priority for the German question led them to oppose a NATO response because it would interfere with detente between Bonn and East Berlin.

With regard to the authorship of the SPD's *Ostpolitik*, Lehmann makes the case for Schmidt. We do not as yet have an equivalent case for Brandt and Bahr, and until we do we must suspend judgment. To this reviewer it seems that the ideas behind *Ostpolitik* were "in the air"; that Brandt, Bahr, and Schmidt expressed variants of them; and that Schmidt's were the more pragmatic and successful.

In any case, Lehmann, who had full access to Schmidt's private archive and, it seems, to much if not all of the SPD archives, provides much material for future historians to draw on. He also gives in exhaustive footnotes what amounts to a survey of the secondary literature, especially in German, on *Ostpolitik*. In view of his overwhelming *parti pris* for Schmidt, however, one must be cautious with his conclusions, which to this reviewer are excessively *pro domo*. This is particularly true of his condemnation of President Carter, which clearly echoes Schmidt's contempt for him. His final treatment of the fall of the Schmidt government is too brief to give much idea of why the SPD opposition to deployment of intermediate nuclear missiles in the

Federal Republic became so strong. Unfortunately, also, his book was completed before he could treat the most recent developments: the differences with respect to West Germany between the Soviet Union, on the one hand, and East Germany and Hungary, on the other. Ironically, Kohl's coalition government has in this reviewer's judgment continued Schmidt's policies the more easily because, like Nixon with China, it is less vulnerable than Schmidt was to attacks from the Right and the Left.

WILLIAM E. GRIFFITH
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RITA BALDI. *Giovan Battista Pigna: Uno scrittore politico nella Ferrara del Cinquecento*. (Quaderni dell'Istituto di Scienze Politiche, Università de Genova; Pensiero Politico, number 2.) Genoa: E.C.I.G. 1983. Pp. 139.

Giovan Battista Pigna, one of the leading men of letters at the court of the last Este duke of Ferrara, is fairly well known as a critic, poet, dramatist, and historian. On the opening page of this book, Rita Baldi claims that there are good reasons for bringing "to the attention of historians . . . [his] political thought" as well. But her readers will be hard pressed to find such reasons in the pages that follow.

Baldi has picked out three treatises that she says "constitute the particularly political production" of her "eclectic" author (p. 53): the *Duello*, the *Pace*, and the *Principe*, and she has summarized the contents of each. She has also gone through the standard surveys of sixteenth-century political literature in search of similar titles by other contemporary authors and has inserted into her summaries the corresponding passages from these authors every time they touch on any of Pigna's arguments. She has thus been able to show that, unlike Girolamo Muzio, Pigna did not disapprove of dueling in all instances and that, unlike Lucio Paolo Rosello, who preferred an elective monarchy, Pigna thought that a hereditary monarchy could more effectively avoid the kind of instability inherent in aristocracies and democracies. If the monarch could not himself be a philosopher, said Pigna, he could always surround himself with philosophers. They would ensure that he concern himself with the quality of the air and the water in his dominions and that he discourage his subjects from concerning themselves with commerce. Realizing a profit, Pigna explained, is not an ethically admissible goal, and ethics always takes precedence over economics.

In an age of mutually antagonistic religious reformations, fratricidal civil wars, pancontinental dynastic rivalries, and overseas expansion, all this sounds a bit trivial. To demonstrate that it was, on the contrary, important, Baldi would have had to familiarize herself with recent scholarship on

sixteenth-century political thought—and not just tell her readers to “see” an occasional arbitrarily selected list of new and old titles (for example, Felix Gilbert’s article of 1939 but not his recent *Choice and Commitment*). She would have had to read the principal monuments of contemporary transalpine political philosophy and something other than just those works of Machiavelli and Erasmus that seem to resemble Pigna’s. Since they attempted to solve problems similar to Pigna’s, she would have had to read all the political works of his compatriots—Giovanni Botero, Scipione Ammirato, Paolo Paruta, Traiano Boccalini, Paolo Sarpi—along with the considerable quantity of scholarly literature that has recently been devoted to them.

But Baldi chose to do none of these things. It may be, to be sure, that the real importance of the three treatises is to be found not in the context of European political thought but in their relationship either to the whole of Pigna’s career as a late Renaissance polymath or to the particular political problems of the state of which he was a subject. But Baldi confines all the “other works” to an introductory vita, wholly isolated from the subsequent opera that interest her, and she is as insouciant of what I recently have said about Pigna as a historian as she is of what Bernard Weinberg long ago said about him as a literary critic. Concerning sixteenth-century Ferrara, she says not a word; the sudden collapse, at the end of the century, of a regime that Machiavelli as well as Pigna thought to be one of the most durable in all Italy remains as much a mystery as ever.

Notwithstanding the official imprimatur of the State Ministry of Public Instruction printed on the *retro* of the title page, the reader will undoubtedly conclude that Baldi has not taken Pigna’s supposedly “political works” very seriously, and he will probably also conclude that Pigna himself did not take them very seriously either.

ERIC COCHRANE
University of Chicago

NOEL MALCOLM. *De Dominis (1560–1624): Venetian, Anglican, Ecumenist, and Relapsed Heretic*. London: Strickland and Scott. 1984. Pp. x, 155. £7.20.

Marcantonio de Dominis was notorious in his own day as a two-time apostate, a sort of Svetlana Aliluyeva playing for much higher stakes. A Jesuit with a promising scientific career, he withdrew from the order in 1597 to assume high ecclesiastical office in his native Dalmatia. In 1616, having resigned as archbishop of Split, he accepted James I’s invitation to come to England. Eight years later he returned to Italy and was welcomed back into the Catholic fold by Gregory XV. Soon thereafter, however, he was

indicted by the Roman Inquisition. His death of natural causes during the trial did not forestall his condemnation for heresy.

What an English pamphleteer of 1624 called de Dominis’s “shiftings in religion” have muddled the waters ever since. Although a few sober examinations of his scientific publications and political theory have been made, full-scale assessments have suffered from *ad hominem* arguments or anachronistic judgments: he has been depicted as a venal opportunist, a Protestant advocate of church-state separation and ecumenism who was ensnared by wily papists, and an early Yugoslav anticipator of socialism. The brevity and unprepossessing appearance of this book (eighty-nine pages of text, offset from a typescript) belie its importance. Noel Malcolm is the first scholar to provide a reasonably full and judicious account of de Dominis’s life and work.

Three of Malcolm’s contributions merit special mention. On de Dominis’s episcopal career in Senj and Split, Malcolm synthesizes recent findings by Yugoslav scholars, thereby rendering a service to historians who cannot read Serbo-Croatian and adding to our knowledge of post-Tridentine diocesan administration in the troubled borderland between the Venetian, Habsburg, and Ottoman states. He deals masterfully with de Dominis’s political theory (which culminated in the three-volume treatise *De republica ecclesiastica* [1617]) by tracing the personal experiences that shaped de Dominis’s stand on secular versus ecclesiastical jurisdiction, distinguishing de Dominis’s theory from those of Paolo Sarpi and James I, and showing how de Dominis’s increasingly adiaphoristic approach to church reunion unwittingly alienated him from Roman Catholics and Calvinistically inclined Anglicans alike. And he persuasively establishes de Dominis’s precise role in the publication of Bacon’s *Essays* in Italian and Sarpi’s *Historia del Concilio Tridentino* (1619). Malcolm thus lays a solid foundation for further studies of this hitherto puzzling, neglected, but important figure.

ANNE JACOBSON SCHUTTE
Lawrence University

WILLIAM A. WALLACE. *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo’s Science*. Princeton: Princeton University Press. 1984. Pp. xiv. 371. \$42.50.

In recent years William A. Wallace has collected a wealth of previously unknown information about the background of Galileo’s thought and work before 1600. Much of this is included in this book, which is thoroughly documented and accompanied by sober scholarly comment and interpretation. The

middle section presents new information about the teachings and the teachers at the Jesuit Roman College (to which professors of natural philosophy throughout Europe looked for guidance) during the formative period of Galileo's science. The final section deals with Galileo's science before and after 1610, the year in which his first telescopic discoveries were announced. The commentary is from a primarily philosophical standpoint.

To have ordered in a single, well-arranged book the fruits of reflection over a quarter-century by a scholar noted for diligence and caution in textual research and analysis is an event of lasting importance to scholarship in several fields. It is striking that this work is in English and is untainted by the patriotic bias often evident in European studies of Galileo and his contributions. No attention, however, is paid to Galileo's measurements and calculations, for Wallace represents an old tradition that spurns utility in science and seeks the origins of science entirely in speculative philosophy.

As a long-term specialist in Galilean research, I find the choice of 1610 as a dividing line unsatisfactory, though commonly made because astronomical discovery is more spectacular than achievement in physics. The crucial dividing year was not later than 1604 nor earlier than 1602, in the opinion of specialists with views as diverse as those of Maurice Clavelin, Winifred Wisan, and myself. In dating one or two highly significant writings by Galileo, Wallace disregards evidence of kinds as telling among historians of physics as are his own kinds of evidence among specialists in philosophical and theological matters. Inattention to the sharp distinction between "dynamics" and "kinetics"—both anachronistic terms—permitted Wallace to reach some conclusions that will not bear careful scrutiny. It is also not true that the cornerstone of Galileo's final book on physics was a mistaken proposition found in his earliest writings on motion (p. 240); on the contrary, Galileo's recognition of that error in 1604 was a principal reason for his abandonment of the idea of "force" as a cornerstone of anything in his mature physics.

Drawing inferences from documentary evidence is the main tool of historians, especially in the history of science. Wallace excels in this in examining the resemblances between the writings of Galileo and those of his contemporaries. Yet other inferences can be drawn from Galileo's own writings as they departed more and more from received philosophical opinion. Striking a plausible balance between inferences drawn in the two different approaches still lies ahead. This book will remain one to be reckoned with in that continuing task.

The book has a valuable bibliography and a detailed index. It is so well printed that I noted only a single unfortunate typographical error: three lines

from the end of page 4, I believe the word "we" should be "he." Even then, the statement seems to me a bit extreme; perhaps "cared" would be better than "knew."

STILLMAN DRAKE
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REINHARDT VOLKER. *Kardinal Scipione Borghese, 1605–1633: Vermögen, Finanzen und sozialer Aufstieg eines Papstnepoten*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 58.) Tübingen: Max Niemeyer. 1984. Pp. xiv, 566. DM 148.

Reinhardt Volker's study is not strictly speaking a biography of Cardinal Borghese or an analysis of his political and social influence. The author concentrates instead on what he sees as essential to an understanding of the cardinal's life and position: his wealth. By a careful reconstruction of the cardinal's finances from a series of account books, Volker intends to outline the financial interests of one of the richest cardinal-nephews of the seventeenth century. He notes that, although the Borghese were more successful than most papal families, the experiences of this nephew of Paul V typified the financial strategies employed by papal families in baroque Rome. Scipione became cardinal-nephew on his uncle's election in 1603. It was through Scipione and his financial, political, and artistic activities that the Borghese family was able to establish a landed patrimony independent of any ecclesiastical office, to build and acquire the palaces and gardens for which they are famous, and to make their way into the upper reaches of the Roman aristocracy.

Basing his procedure on Norbert Elias's *The Court Society*, Volker argues that to divide expenditures into investments and consumption is not quite accurate since expenditures for servants, religious feasts, pensions, and so forth were essential if one wished to acquire or maintain status. But he does believe that in a typically bourgeois way Scipione Borghese carefully moderated consumption in order to avoid bankruptcy, the fate that haunted most old aristocratic families. The problem was, of course, that a pope might live for only a few months, giving a family little time to acquire and consolidate wealth. Thus, the initial strategy was to get as much as possible as quickly as possible and then to shift the financial base away from purely ecclesiastical sources. Finally, the family had to create a balanced portfolio.

Volker distinguishes six progressive stages in Borghese's financial career. Initial real investments, like the purchase of a villa in the Frascati hills, were for status. Only later did Scipione shift toward income-producing property and financial speculation, the twin foundations of the independent for-

tune Scipione had built by his uncle's death. The most important source of revenue was a series of abbeys held in commenda, mostly in Lombardy. They represented, at times, 50 percent of the Borghese income. Pensions, rents, and profits from lands produced 20 percent, and the rest came from a combination of sales of goods and outright gifts from the papacy. The death of Paul V brought a dramatic change. After an initial flurry of high-status investments, nonrevenue-producing expenditures were moderated, and Scipione emphasized the purchase of rental property. Volker concludes that the successful cardinal-nephew had to plan carefully and maintain constant control of expenditures. The creation of a patrimony independent of ecclesiastical office was essential, and, not surprisingly, land was the surest way to do this.

Volker has been thorough. Tables, balance sheets, and excurses on various properties and offices allow one to establish with accuracy Borghese finances and to measure reaction to economic change. Yet the book remains frustrating because the villa in Frascati, the famous gardens on the Pincian, and the churches and palaces in the Quirinal remain abstract financial data. The power and influence of Scipione remain potential and not actual. Volker concludes that the cardinal-nephews provided the facade and structure for the development of a Roman version of the justly famous "court society" of the seventeenth century. One wishes that the dynamics of that court and not just its financial base had been the focus of the author's attention.

DUANE J. OSHEIM
University of Virginia

GAETANO CINGARI. *Storia della Calabria dall'unità a oggi*. Rome: Laterza. 1982. Pp. 498. L. 34,000.

Despite more than a century of unity, Italy retains regional and sectional differences. Indeed, unification, which occurred when industrialization was taking hold in northern Italy, itself created new regional problems. Industrialization represented progress, and the rulers of newly united Italy, eager to attain great power status, subordinated other economic demands to industrial growth and expansion. Calabria was one of the regions most disadvantaged by these policies, as this admirable study amply documents. Gaetano Cingari, a former Socialist deputy in the Italian Chamber and the vice-president of the Regional Council for Calabria, traces this region's vicissitudes since unification. In eleven chapters he identifies and analyzes the major political, social, and economic trends of these years.

Covering the southwest corner, or the toe, of peninsular Italy, Calabria is three-quarters mountainous or hilly. Under the Bourbons, artisan cot-

tage industries producing raw silk for export and a variety of products for local consumption flourished. Salt mines at Lungro and iron works at Mongiana represented a small but promising industrial activity. After 1861, however, a combination of factors influenced a decline in the region's economy, which became increasingly agricultural and pastoral. With the rest of southern Italy, Calabria became a problem area.

Suffering from one of the highest rates of illiteracy in Italy, the population responded to the new administration first with brigandage and then with emigration. Floods, in large part the result of poor land management and earthquakes, increased Calabria's malaise. Only with the establishment of the *Cassa per il Mezzogiorno* after the Second World War did some of the basic problems of a steadily deteriorating economy begin to be addressed.

Never completely won over by fascism, as it had not been by the liberal regime, the Calabrian electorate has surprised political pundits since 1947 by diverging from the rest of southern Italy. In national referenda it has supported the republic, divorce, and abortion, positions seemingly at odds with the region's cultural and intellectual sophistication. Ever below the surface, popular unhappiness and discontent flared into riots at Reggio from July 1970 to early 1971 over the siting of the regional capital.

More than a history of Calabria since unification, Cingari's book is a history of Italian governmental policies in these years. His well-documented study clearly brings out that the country's elite grievously failed to meet the needs of Italy's poorer and less industrially developed regions. While the facade of a modern state fronted the world, especially during the fascist years, the core remained weak, and profound socioeconomic and political imbalances continued to divide the Italian people.

Based on an eclectic and exhaustive variety of sources, which include government reports and statistical surveys on southern Italy, parliamentary papers, vernacular poetry, and the writings of Corrado Alvaro (contemporary Calabria's leading writer), this study is comprehensive without being didactic or superficial. The omission of maps to supplement the text is, however, a serious drawback, but the book's breadth and scope compensate for this weakness and make it a model study. It is to be hoped that the publisher will follow with similar volumes on the other regions of Italy.

EMILIANA P. NOETHER
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WILLIAM A. DOUGLASS. *Emigration in a South Italian Town: An Anthropological History*. New Brunswick, N.J.: Rutgers University Press. 1984. Pp. xvi, 283.

In this intensively researched volume William A. Douglass examines emigration from Agnone, a rural administrative center in the mountainous region of upper Molise. About equidistant from Rome and Naples but easily accessible to neither of them, Agnone once prided itself on being the "Athens of Samnium," and today its only healthy industry is education.

Students of Italian history are likely to find the book's historical and anthropological approach refreshing. Douglass obviously is concerned with theory, yet he is not overly burdened by commitments to model building and testing. Rather, the Agnonese for the most part tell their own story. And, if the author occasionally assists them with an interpretation of their smiles or an explanation of the economic significance of lowering one's pants, we want such insights from an anthropologist. Particularly effective is his treatment of the nineteenth-century liberal dilemma and ultimate failure of attempting to maintain *nouveau riche* privileges while granting at least a minimally viable economic life for the vast majority of townspeople from the artisan and peasant classes. When enough workers finally left, the parasites had to find other hosts. Perhaps less satisfying is the analysis of the earlier feudal period. The details provided are by no means uninteresting or unimportant, but the sometimes vague allusions to generalizations about Western feudalism in place of explicit references to works by Giorgio Giorgetti, Emilio Sereni, and other scholars who have closely studied Italian agriculture weaken the author's conclusion that the Agnonese's cosmopolitan past and physical mobility prepared them especially well for their transatlantic ventures in the late nineteenth century. He describes a chronological sequence of events but does not test rigorously the implied causal pattern. The tragedy of Agnone's acquiescence in fascism is told in uncompromising fashion, whereas its postwar decline seems an all-too-familiar and fatalistic story told in the liberal, Christian manner.

The book's most important contribution, at least in my judgment, is to the wider audience of scholars concerned with immigration to and ethnicity in North and South America. Douglass describes the Agnonese who are now spread over five continents as a "diaspora." The word captures nicely the interconnectedness of a people separated by geographical barriers, class lines, and time itself yet bound by their common identity as Agnonese. The ties of birth and blood in this instance are not reinforced with a sacred text, although the town newspapers and local histories, an unusually rich collection, might well be read in this way. Within this humus of emotional pressure and the struggle merely to survive, potential migrants formulated life strategies and saw relocation as a process rather

than as an event. And, like their fellow *paesani* around the world, they constantly changed their circumstances and, thus, their opportunities by their own decisions. Thus, Douglass confirms and adds some new shading to the portrait of a rational, informed peasant-strategist painted over the past decade by scholars of the Italian migrant.

RUDOLPH M. BELL
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RAFFAELLA GHERARDI. *Le autonomie locali nel liberismo italiano, 1861-1900*. (Studi e Testi, number 8.) Milan: Giuffrè, for the Istituto per la Scienza dell'Amministrazione Pubblica, Milan. 1984. Pp. xv, 263. L. 22,000.

Recently Italian historiography has paid increased attention to the interrelationship between administrative organization and economic and financial questions. This book, which focuses on the theoretical elaborations and political strategies of the free trade school in the peninsula during the years from 1861 to 1900 when the Liberals were in power, is a welcome addition to that literature. Specifically, the study examines the reaction of two important political-economic reviews—*L'Economista* and the *Giornale degli economisti*—to the various proposals to reorganize the relationship between local government and the central state.

The preface, by Giorgio Berti, and the introduction, by Raffaella Gherardi, trace the origin of the study, discuss its significance, and place what follows in perspective. The main body of the monograph is divided into two sections that treat the reaction of *L'Economista* and the *Giornale degli economisti* to the issue of centralization versus decentralization as well as to a host of related matters. The chapters in part 1 deal with the political liberalism of Francesco Ferrara, the obstacles to communal authority in the years of transition from Right to Left, as well as the reaction of *L'Economista* to the proposals to reorganize relations between the state and the local entities.

In the second half of the work Gherardi traces the reaction of the *Giornale degli economisti* to the division of administrative responsibilities between state and local authorities. She provides a history of the formation and evolution of the review, which helps explain its position. The journal, which began publication in April 1875 under the inspiration of Fedele Lampertico, Luigi Cossa, and Luigi Luzzatti, reflected the sentiments of these "socialists of the chair" and called for state regulation. At the end of 1878 the Paduan period of the review concluded, as did its role as official organ for the interventionist Association for the Progress of Economic and Social Studies. Following its move to Rome in 1890, when Alberto Zorli was joined by Maffeo Pantaleoni and

Antonio de Viti de Marco as codirectors, the journal adhered to marginalism in economic theory and free trade in political economy. It abandoned its earlier call for social legislation and state action and relied on "economic science."

During the Paduan period, Luzzatti, one of the key figures of the *Giornale degli economisti*, wrote some fifty articles for the journal in the space of three years. In these he touched on the question of decentralization, noting that, although the notion of an infallible government was absurd, no less false was the belief that the commune could not err. During the subsequent series of the journal in Bologna and Rome, when there was a decided shift against the intervention of the state in economic and social matters, the journal remained cautious on the question of local autonomy. Its writers such as Giulio Alessio, who cited the need for freedom in local government, still saw the need for state supervision. In the articles in both reviews one finds considerable ambivalence on the question of how best to secure local interests.

An analysis of both the editorial comments and the individual articles in the journals reveals continued debate about the extent and mode of control of the central government over local institutions and authorities, about whether the small and large communes should be treated alike and whether their syndics should be appointed by the central government or elected by the communal councils. Aware of the contradictions and the inability of individual articles to form a coherent program, the author has provided considerable analysis to clarify the positions of the two journals. Unfortunately, the wide range of articles examined and the diverse number of projects considered within the pages of the journals do not make her task an easy one. In her conclusion Gherardi attempts to tie the loose ends and disparate pieces together. She succeeds only in part.

FRANK J. COPPA
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DAVID I. KERTZER. *Family Life in Central Italy, 1880-1910: Sharecropping, Wage Labor, and Coresidence*. New Brunswick, N.J.: Rutgers University Press. 1984. Pp. xvii, 250. \$32.00.

In this otherwise persuasive study it is the title's reference to central Italy that perplexes. The population data underlying the original part of the research refer to the parish of Bertalia, once a rural community that is now well within Bologna's urban sprawl, just a few kilometers northwest of the city center. Bertalia thus sits right in the middle of the Po Valley, well to the north of the Apennine water-

shed that marks the boundary between northern and central Italy. David I. Kertzer knows this perfectly well but chooses nevertheless to consider this part of the Po Valley as an extension of central Italy on the ground that between 1880 and 1910 the agriculture of both areas was characterized by a mixture of sharecropping and labor hired on a daily basis. By 1880, however, the agricultural population of Bertalia already consisted mostly of day laborers, whereas the reverse was true of the central regions of Marches, Tuscany, and Umbria, where sharecropping prevailed. It would have made more sense to study sharecropping in the regions where it prevailed rather than where it was already a declining form of land tenure, for it is risky to assume that the presence of a large landless labor force and a rapidly changing regional economy affected family life in ways that were typical of sharecropping in general.

By looking at a region that was experiencing rapid economic and social transformations, however, the author is able to draw some instructive comparisons with other parts of Western Europe. Had he looked at the real central Italy, where the commercialization of agriculture, urbanization, and industrialization were more muted, he might have given us a more convincing description of sharecropping but would probably have lost some of the comparability of his data, and Kertzer intends to place the Italian case in the mainstream of current debates over changing patterns of coresidence in Western Europe. The message that emerges from the many graphs, tables, and charts and from the usually clear explanations of their significance seems to confirm and clarify what we already knew from less quantified studies and sources. On the average, sharecropping families were indeed larger than the families of day laborers, mainly because their relationship to the land made it economically advantageous for them to establish multiple family coresidence on the basis of "patrilocal postmarital residence" whereby sons brought their wives into the parental household (p. 159). Expanding beyond local boundaries, Kertzer goes on to suggest that we must carefully qualify the recent claims by Peter Laslett and others that the extended family in Western Europe's past is mostly a myth. According to Kertzer, extended families were common in regions of sharecropping and small landownership, and these certainly include not only Bologna but also central Italy and much of southern France. Furthermore, Kertzer demonstrates that at least in Bertalia extended families remained common well into the period of urbanization and industrialization. He thereby arrives at the important conclusion that distinctions between rural and urban areas are less important in influencing family patterns than the

nature of the "access to the means of production" (p. 101).

ROLAND SARTI
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ROMANO FERRARI ZUMBINI. *L' "Incidente" Nasi: Cronaca di una vicenda dell'Italia politica d'altri tempi, 1903-1908*. Padua: CEDAM. 1983. Pp. x, 179. L. 12,000.

According to the constitution (*Statuto*) of the Kingdom of Italy, government ministers who were impeached by the Chamber of Deputies were to be tried by the Senate sitting as a high court of justice. Until 1907 no such proceeding had occurred, nor was one to recur so long as the *Statuto* lasted. In that year, however, Nunzio Nasi, former minister of education (1901-03), was tried by the high court on charges of repeated peculation, found guilty in 1908, and sentenced to eleven months and twenty days of confinement.

The charges against Nasi surfaced in the chamber at the end of 1903, soon after the resignation of the government of which he had been a part. In 1904 the chamber authorized his arrest and trial before an ordinary tribunal. But in 1907 the Roman Court of Cassation ruled that the ordinary judiciary was not competent to try a former minister for acts committed while in the exercise of his office, whereupon the chamber deferred Nasi to the high court.

The Nasi "incident" raised a host of legal and political questions. The chief merit of Romano Ferrari Zumbini's brief but carefully prepared work lies in its examination of the constitutional and juridical aspects of the story and in its awareness of the exquisitely political nature of so unique a case. The structure of Zumbini's book reflects this dual interest. The first chapter, which is appropriately entitled "Political Justice" and accounts for about two-fifths of the work, is devoted to a detailed examination of historical precedents as well as Italian and foreign constitutional and legal provisions for proceedings against government ministers. The balance of the book, rich in detailed citations of sources and explanatory footnotes, deals with Nasi's case proper in its legal and political aspects, viewed severally and in their inextricable interweaving.

An extraordinary confluence of events contributed to the production of the "incident" that, within a few years, destroyed the career of a rising political figure whose power base in western Sicily, Masonic affiliation, and apparent royal favor all promised higher and even the highest office. But chance and circumstance, in the form of the ambitions of others and his own failings, intervened. Vittorio Emanuele Orlando, a fellow Sicilian and future prime minister, seemed to stalk Nasi's political footsteps;

Vincenzo Saporito, another Sicilian, became a nemesis; Giovanni Giolitti, prime minister before, during, and after the trial, may have viewed him as a rival; and Antonio Salandra, another future prime minister, was less than friendly toward Nasi. Distinctly unfriendly was a major newspaper, the *Giornale d'Italia*, which in turn was friendly to Salandra. Finally, the Socialist party and some Socialist notables found the Nasi case a political convenience to exploit.

Zumbini questions neither Nasi's guilt nor the justice of the verdict that ended the politician's career; he does raise questions about the nature of political justice as rendered by the Senate sitting as a high court; and he marvels at the coincidence of rivalries, hostilities, and ambitions that surrounded the "incident."

SALVATORE SALADINO
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MICHEL OSTENC. *Intellectuels italiens et fascisme, 1915-1929*. (Bibliothèque Historique.) Paris: Payot. 1983. Pp. 338. 120 fr.

Michel Ostenc, the author of an excellent comprehensive work on fascist education (*La scuola italiana durante il fascismo* [1979]), has deliberately not attempted to write a general history of intellectuals under fascism. Instead, his study concentrates on the evolving political attitudes of certain writers and poets like Gabriele D'Annunzio, Filippo Marinetti, Giuseppe Ungaretti, Luigi Pirandello, Curzio Malaparte, and the antifascist Piero Gobetti. He wisely terminates his study in 1929, because, despite the differences in their ages, the men studied all belong to the early period of Italian fascism and not to the new era that opened around 1930.

The author reveals the tremendously diverse ideals that drew intellectuals to fascism. The futurist Marinetti desired to break with a corrupt past by a leap into the new industrial and technological future; Ungaretti hoped that an escape from the corruption of the present might be found in a return to Italian classical traditions; and Malaparte sought reconciliation between the elite and the masses by means of fascism. Yet Ostenc shows that the intellectuals who were drawn to fascism shared a common outlook on politics. They were all nationalists who believed that liberal Italy had failed to create a moral unity for the Italians. Although the nation was flawed and incomplete, the solution was not economic development but a cultural awakening. Fascism was to be a moral revolution that would provide some missing piece in the Italian character. Finally, they all sought in Mussolini a leader under whose direction the changes might take place.

Ostenc reveals how the framework for this political activity was set by an older generation led by Benedetto Croce and Giovanni Gentile, who, before World War I, directed their most severe attack against Marxism, reformism, and democratic values. Just before and during World War I a new generation fused this philosophical assault with a romantic vision of politics that lacked any basis in Italian reality. Their Italy was a myth. The author then demonstrates convincingly that fascism was never able to shape these disparate ideals into a serious and coherent doctrine.

Most of the figures studied were creative writers, which may have distorted the sample somewhat. D'Annunzio is given an inordinate amount of attention, for he had little real importance after 1921 and played no role under fascism. Similarly, Pirandello's case was so special that his adherence to fascism throws little light on the broader general question of intellectuals and fascism. But these are minor points. Ostenc's chapters on the relationship between the intellectuals and the new fascist movement are very well done, and the conclusion to that section is a fine summary of the whole problem. The author's judgment that fascism managed to construct a formal cultural apparatus but had no content to put into it is solid. Ostenc has made a serious and valuable contribution to the growing body of work on Italian intellectual life from 1900 to 1929.

ALEXANDER DE GRAND
Roosevelt University

LINDA FREY and MARSHA FREY. *A Question of Empire: Leopold I and the War of Spanish Succession, 1701-1705*. (East European Monographs, number 146; Brooklyn College Studies on Society in Change, number 36.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1983. Pp. ix, 165. \$20.00.

Although the reign of Leopold I has long been considered a decisive period in the consolidation of the Habsburg *Gesamtstaat* in Central Europe, the emperor himself has customarily been described by historians as a ruler so uncertain of his own abilities, so disinterested in politics, and so preoccupied with inconsequential matters that he seldom acted decisively and fell time and time again under the influence of confessors and court favorites. A papal nuncio anticipated the view propounded by several generations of modern scholars when he observed that Leopold was much too pious to be emperor. Only recently has this interpretation been challenged by specialists who have stressed Leopold's intelligence, determination, and devotion to duty and insisted on the importance of his personal role

in the emergence of the Danubian monarchy as a great continental power.

For those who have read the spate of articles by Linda Frey and Marsha Frey on various questions of diplomacy, war, and rebellion in the early eighteenth century, their refinement of the new approach in a study treating the numerous issues raised by Leopold's involvement in the Spanish succession will come as no surprise. Regarding the emperor as a reasonably pragmatic statesman and worthy protagonist of the Sun King, they argue that he vacillated on crucial issues not so much because he was passive and irresolute by nature but because he often saw in equivocation a means by which he could enhance his influence over rapidly unfolding events. From Leopold's perspective, the lands of his Spanish cousins formed an inalienable part of his dynastic inheritance, and in the last years of his reign he never wavered in his determination to assert the territorial claims of the House of Habsburg or to counter French aggression with every means at his disposal.

The Freys' analysis of Leopold's policies in his final struggle against Bourbon hegemony offers a revealing perspective on the inescapable problems involved in waging coalition warfare. In Leopold's case these problems stemmed ultimately from divergent assumptions, interests, and objectives among the various allies, but they were complicated by the emperor's reputation as a bigoted Catholic and ruthless tyrant—a reputation that became especially pronounced following the outbreak of the Rákóczi rebellion. Stereotyped images of the way he treated Protestants developed a life of their own and had a direct bearing on his relations with the maritime powers. Although the Freys offer relatively few original insights into the problem of the Spanish succession, they do demonstrate the impact of particular personalities and the central place of factional strife on the workings of the Vienna court. They are also able to show that 1703 was a critical turning point in Austria's connections with England and the United Provinces. Given their painstaking research, based on an extensive investigation of archival materials and admirably reflected in two helpful appendixes, detailed notes, and an immense bibliography, it is unfortunate that the Freys have conveyed the fruits of their scholarship in a main text marred by distracting typographical errors and a persistent misuse of commas as well as by a needless repetition of key phrases and basic themes.

JOHN A. MEARS
Southern Methodist University

ANNA M. CIENCIALA and TITUS KOMARNICKI. *From Versailles to Locarno: Keys to Polish Foreign Policy*,

1919–25. Lawrence: University Press of Kansas. 1984. Pp. xvii, 384. \$29.95.

In light of what happened to resurrected Poland, most historians have been highly critical of those who charted that country's foreign policy between the wars. Anna M. Cienciala and Titus Komarnicki attempt to soften this verdict by explaining how Polish leaders saw their responsibility in the inherently difficult situation after World War I. The authors' contention is that interwar Poland was not really mistress of her own fate but subject to the whims of Western policy, which reflected little understanding for Polish problems and interests. That this is as much an account of Anglo-French foreign policy as of Polish only underscores this point.

This book complements the recent work of Kay Lundgreen-Nielson on the Polish problem at the Paris peace conference and picks up where that work left off, focusing especially on the territorial disputes of the following years and concluding with a quite negative assessment of what the Locarno Pact meant for Poland. This period of European international relations has been treated many times before, but the Polish point of view has not previously been presented this well in English.

The two authors present some sharp contrasts: Komarnicki (1896–1967) was a diplomat, a functionary of the government whose policies he defends here; Cienciala is a professional historian from a different generation. Cienciala seems to have reworked quite thoroughly the manuscript she "inherited" from the older scholar-diplomat; frankly, one wishes that she had rewritten it entirely, for the several chapters that (one conjectures) bear Komarnicki's stamp are the weakest, characterized by nationalist special pleading and old-fashioned partisanship. The treatment of the Upper Silesian plebiscite of 1921 is especially one-sided, attempting to explain away the vote of 60 percent of the population for Germany with references to German "terror" and British economic greed, despite circumstances (Polish insurrections, a French occupation force) that worked more in Poland's favor. The historical background provided for the Danzig problem—suggesting that the city's German character (96 percent) was recent, partial, and superficial—is similarly distorted.

Even allowing for the fact that the purpose of this book is to explain and, by implication, to defend Polish policies, one must question its consistently uncritical attitude. The authors embrace the untested assumption that any attempt by Poland to coexist with her two powerful neighbors was senseless and could only be achieved at the price of her independence. Almost any nationalist posture or appetite for territory is justified with reference to these hostile neighbors; when Polish leaders refuse

even manifestly reasonable proposals (for example, autonomy for eastern Galicia), this is because Polish public opinion, the court of last resort, would not tolerate them. The contention that this attitude weakened rather than strengthened Poland, substituting mere size for internal cohesiveness and making bitter adversaries of her four principal neighbors, remains unexamined. To conclude that "Polish statesmen achieved what was possible" (p. 279) ignores the obvious failure of their policies and the terrible price Poles have been paying for this failure since 1939.

But the fact that this reviewer has some differences of opinion with the authors does not lessen his respect for this book's scholarship. It may be what some today call "traditional diplomatic history," but it is done well. Based on exhaustive research in the archives of a half-dozen countries, it is a thorough but lucid account of a series of significant, complex events. By making the case for Poland's leaders, it may prompt some adjustments and thus accomplish its purpose.

RICHARD BLANKE
*University of Maine,
Orono*

MICHAEL M. BOLL. *Cold War in the Balkans: American Foreign Policy and the Emergence of Communist Bulgaria, 1943–1947*. Lexington: University Press of Kentucky. 1984. Pp. ix, 250. \$25.00.

Michael M. Boll has written an informative study on American policy toward Bulgaria during the last years of World War II and the early postwar period. His book not only describes the evolution of American policy toward that troubled state but also reveals the dynamics of domestic Bulgarian politics during this critical period. The result is a balanced and insightful account of the inability of American policy to prevent the Sovietization of Bulgaria.

During the closing years of World War II, the United States took the initiative in trying to secure a Bulgarian surrender and looked toward the establishment of an independent democratic government with a pro-Western orientation. Since the Bulgarians had not declared war against the Soviet Union—Bulgaria was unique among Hitler's allies in not doing so—the Soviets were not directly involved in what was an Anglo-American affair. While the Bulgarian government temporized on the issue of surrender, the Soviets occupied northern Bulgaria in September 1944, thus upsetting America's goals for Bulgaria's future. Now the Soviets and their cronies, the Bulgarian Communists, became the new political realities confronting American efforts to assert Western influence in Bulgaria.

As Boll clearly demonstrates, the future of American interests in Bulgaria did not appear very promising after the entry of Soviet armed forces into the country. Soviet domination of the Allied Control Commission, headed by General Sergei Biriuzov, justifiably angered Maynard B. Barnes, the chief American political representative in Bulgaria, and Major General John A. Crane, the official American representative to the Allied Control Commission in Bulgaria. Even more disturbing was the success of the Bulgarian Communists, who grabbed a major share of the country's political, military, and economic positions, which gave them enormous advantages in towns and villages.

Boll challenges the idea that all this was part of a master plan to convert Bulgaria into a Soviet satellite. He sees the eventual Sovietization of Bulgaria as a process, not something preordained by Moscow. He points out that Stalin was preoccupied with military matters in 1944 and early 1945, especially with stabilizing the fluid political and social situation in Bulgaria while the Soviet Union was still at war with Germany. If Stalin's policy was indeed in a state of flux during the closing months of World War II, it was obvious that possibilities for the United States to assert its political influence in a country occupied and controlled by the Soviet Union were quite limited.

The United States adopted a policy of nonrecognition of the Communist-dominated Bulgarian regime and refused to sign a peace treaty until the government had been reconstituted to include democratic elements. The American intention to have some role in the determination of Bulgaria's political future was dramatized in the spring and summer of 1945 when the Bulgarian Communists, better organized than their divided opposition, planned to hold elections based on a single electoral list. Since the proposed elections would have resulted in a Communist victory, the United States intervened and forced their postponement, achieving what proved to be an ephemeral success. As the prospects of democratic reform in Bulgaria and improvement in United States-Soviet relations deteriorated, Washington abandoned its nonrecognition policy. By 1947 it ended up signing a peace treaty and establishing diplomatic relations with the Sofia regime in the hope of securing only a small amount of influence in Bulgaria. As Washington soon discovered, America's new policy was no more successful in stemming the tide of communist totalitarianism in Bulgaria than its previous policy had been.

Boll suggests that the abortive Foreign Ministers Conference in London, held late in 1945, appears to have been the turning point in Soviet policy toward Bulgaria. He claims that American insistence on asserting influence in the Balkans while denying Soviet demands for similar influence in areas under

American occupation allegedly convinced the Soviets to press on toward complete domination of Bulgaria, a process the United States was unable to prevent.

RICHARD C. LUKAS
Tennessee Technological University

GEORGE P. MAJESKA. *Russian Travelers to Constantinople in the Fourteenth and Fifteenth Centuries*. (Dumbarton Oaks Studies, number 19.) Washington, D.C.: Dumbarton Oaks. 1984. Pp. xvii, 463. \$28.00.

George P. Majeska provides historians of Byzantine and Muscovite Russian culture with a superb edition of five travelers' descriptions of voyages to Constantinople. Taken together, these pilgrim tales constitute a treasure house of information about the architecture, life style, and topography of late Byzantine Constantinople.

Majeska edits, translates, and comments on *The "Wanderer" of Stephen of Novgorod* (ca. 1349), *The Journey to Constantinople of Ignatius of Smolensk* (ca. 1390-92), *The Anonymous Description of Constantinople* (late fourteenth century?), Alexander the Clerk's *On Constantinople* (1394-95), and Zosima the Deacon's *Xenos* (1419-22). The editions meet the highest scholarly standards: Majeska attempts to reconstruct the authors' original texts from virtually all preserved manuscripts and augments his reconstructions with a complete critical apparatus for verification or alternative reconstructions. In addition, the translations read well and are true to the "original" reconstructions. Majeska has indeed set a standard that all scholars of the medieval Slavic world should follow.

The introductory commentaries provide excellent historical settings for the pilgrim tales and for the evolution of the manuscript traditions pertaining to each text. Historians of both late Byzantium and Muscovy will welcome the ways in which Majeska creates context for his most fascinating texts.

Especially useful are his commentaries on the city of Constantinople, the architecture of the city, and political events between 1390 and 1392, including the uprising of 1390, the taking of the imperial city by John VII Besieges, and the coronation ceremony for Emperor Manuel II in 1392. For these commentaries, which constitute part 2 of Majeska's book, the author relies on non-Slavic sources to evaluate the authenticity of his primary texts.

The book is first-rate. If this reviewer had to be critical, she would simply lament the omission of lengthy commentary on the attitudes toward Byzantium of West Russian and Muscovite travelers at the turn of the fifteenth century as reflected in the texts. She would also lament the omission of scholarly material published after 1977 owed, of course,

to the pace of publication of critical editions and commentaries in this country.

Russian Travelers should be purchased by all university libraries, by all Byzantinists, and by all historians of Muscovite culture.

ELLEN S. HURWITZ
Lafayette College

NICKOLAS LUPININ. *Religious Revolt in the XVIIth Century: The Schism of the Russian Church*. (Men and Movements in Religious History, number 1.) Princeton, N.J.: Kingston. 1984. Pp. 227. \$24.00.

Foreigners often mock the Russian Orthodox faith, mores, rituals, and public forms of piety. The long history of that scorn has left deep wounds and nourished resentment. Nowhere has the critical relationship been more evident than in the unsettled period of the mid-seventeenth century, when Russia's vital religious culture encountered changes coming simultaneously from the West and the Orthodox East, producing a profound crisis. Spiritually unsure of themselves, Russians fell prey to upheaval and schism. The crisis left the country weakened and subject to the subsequent shocks of Western secularism. In brief, this is the theme of Nickolas Lupinin's book on the religious revolt within the Russian church in the seventeenth century.

The book divides into two roughly equal parts. The first portrays a religiously dominated Muscovy going through a crisis of change congruent with the Western religious crisis of the same time. Lupinin surveys the material and ecumenical position of the Russian church before the schism of 1666-67, listing and describing various aspects of Russian religiosity that in his view indicate how thoroughly the church suffused traditional Muscovite life. This discussion is followed by a survey of domestic and foreign changes affecting the relationship of church and society. The second part focuses on the personalities and issues of the schism. The tempestuous Patriarch Nikon forced change, transforming a gradualist reform movement into one of abrupt confrontation. Lupinin then shifts attention to the Old Believers, the gradualists, their approach to reform before condemnation by the Great Council of 1666-67, and their subsequent reaction to the events of those years. Having dealt with the personalities, Lupinin concentrates on Nikon's reforms, which he argues are "essentially textological [sic]" (p. 128). Chapter 8 chronicles the schism from its inception in 1653, when Nikon, following his elevation to the patriarchal throne the preceding year, could insist at last on his reforms. Lupinin concludes that after a quarter-century of resistance, culminating in a tragic seige (1668-76) of the Solovetskii

monastery in Russia's far north, the Old Believer leaders in a sense emerged victorious, for thousands of Russian Orthodox were persuaded to choose the "real church," as they understood it.

The Great Council is, in the author's view, the "key" to Russia in the seventeenth century because its critical sessions, dominated by haughty and arrogant Greek churchmen, upheld Nikon's reforms (while condemning the patriarch), thereby breaking with Muscovite tradition and dividing the Orthodox faithful. Tsar Alexis thereafter found it possible to dominate the church in ways portending the new Western and secular Russia of Peter the Great. Lupinin does not widen significantly our knowledge of the period or present much that is new by way of interpretation. In fact, his conclusion that the religious revolt of the seventeenth century prepared the way for Peter's "secularization" of the church comes at a time when historians of Orthodoxy in imperial Russia (especially Gregory Freeze) are revising the standard view.

The book is carelessly edited (typographical and other errors appear on nearly every page), the glossary of terms is hampered because many terms peculiar to the Orthodox faith employed in the text are omitted, and the index is incomplete and marred by misspellings.

ROBERT L. NICHOLS
St. Olaf College

L. P. SHOROKHOV. *Korporativno-votchinnoe zemlevladienie i monastyrskie krest'iane v Sibiri v XVII-XVIII vekakh: Razvitie feodal'nykh otnoshenii i ikh osobennosti* [Corporative-Patrimonial Landownership and Monastic Peasants in Siberia in the Seventeenth and Eighteenth Centuries: The Development of Feudal Relationships and Their Features]. Krasnoyarsk: Krasnoyarsk University. 1983. Pp. 162. 1 r. 50 k.

Recent decades have seen an impressive body of thorough and often pioneering research by Soviet historians of Siberia. Studies of settlement, peasant life, the exile system, and even Siberia's modest cultural life have provided a picture that is often much more detailed than the one we have of European Russia. At the same time many useful, if more traditional, writings have continued to appear, such as the present study of the peasants of monastic and episcopal estates in the seventeenth and eighteenth centuries. L. P. Shorokhov discusses three themes: the rise of monastic and episcopal estates, the history of the peasantry of those estates and their relations with their masters, and the effects of the secularization of ecclesiastical estates in 1764.

In Siberia, as in the Russian North, gentry landholding did not exist, and the only lords were those of the church. It seems that in Siberia church estates

were smaller than those west of the Urals, containing only about half as many peasant households. Pointing to the church's recovery of runaways and the existence of labor services, Shorokhov argues that the status of these peasants was essentially servile. It is difficult to believe, however, that local conditions were so unimportant. Perhaps the problem is that the author relies entirely on records of estates and does not try to set ecclesiastical villages in the larger context of Siberian life. He also follows the older generation of Soviet rural historians in assuming "state" peasants to be little different in status from gentry-owned serfs, a dubious proposition that writes off the local conditions of the North and Siberia. Even so, he concedes that the secularization of 1764 did produce a modest improvement in the productive capacity (and presumably living standard) of the peasants.

If rather conventional within its tradition, Shorokhov's work is not without merit. He does not merely illustrate his views but also tries to give a complete and systematic account of the various estates and their populations. In this attempt he largely succeeds, and the book will be the basis for any further exploration of the subject.

PAUL BUSHKOVITCH
Yale University

NICHOLAS V. RIASANOVSKY. *The Image of Peter the Great in Russian History and Thought*. New York: Oxford University Press. 1985. Pp. ix, 331. \$29.95.

Until now the work of Nicholas V. Riasanovsky has concentrated mainly on the late eighteenth and early nineteenth centuries. His study of the image of Peter the Great greatly expands these bounds, moving from contemporary accounts of the famous reforming tsar to the latest Soviet monographs. Space is given only to works by Russians living in Russia; the large body of work done in the West by Europeans, Americans, and Russians in emigration is ignored by explicit design, although occasional references are made to important Western works in the footnotes.

The study is divided into four chapters: the Russian Enlightenment, 1700–1826; the Age of Idealistic Philosophy and Romanticism, 1826–60; the Age of Realism and Scholarship, 1860–1917; and the Soviet Union, 1917–84. This instructive breakdown is compromised, however, by a somewhat mechanical organization: each chapter receives almost exactly the same amount of space, as if all were of equal importance. Yet the two middle periods are obviously far richer than the outer two in their interpretive range and implications.

Riasanovsky begins the book with his own summary of Peter's image, and a surprisingly uncritical

one it is. He mentions in passing that Peter "all his life . . . struggled against his emotions, rage especially, but repeatedly lost that struggle" (p. 5). Yet he ends the summary with a highly positive judgment: "[Peter was] a man of action rather than a man of thought; a practitioner, not a theoretician. He was also something of a visionary. With grandeur and optimism, themselves typical of the age, he foresaw the image of a modern, powerful, prosperous, and educated country; and it was to the realization of that image that he dedicated his life" (p. 10). This strong affinity for Peter as the founder of modern Russia's greatness lies just beneath the surface in much that follows, and it is difficult to measure the extent to which it influences the picture we receive.

Riasanovsky summarizes the views of each major figure in Russian literature and historiography on Peter and his work. He makes a convincing case for the rapid establishment in the eighteenth century of a kind of cult of personality for Peter. This was modified during Catherine II's reign by the empress's admirers, who sought to contrast the more gentle and humane spirit of her court with the brutalities of the early years of the century. Most significant, however, is the treatment of the first half of the nineteenth century. The texture here is rich with much attention being given to the delicately nuanced and ambivalent stance of the genius Aleksandr Pushkin. A special place is also reserved for Petr Chaadaev, whose view of Peter the Great shifted from the first "Philosophical Letter," which dismisses the tsar, to the "Apology of a Madman," which became a wellspring of both the Westerner and Slavophile positions. As Riasanovsky explains, the Slavophile treatment provided the decisive break with the previous era—a break made possible by the new categories of thought brought in with the shift from the intellectual world of the Enlightenment to that of romanticism. National pride could now be conceived of in opposition to the powerful image of Peter the Great.

The two final chapters are devoted almost exclusively to the views of historians and therefore lack the importance of the earlier chapters for the cultural history of Russia more broadly. The shifting interpretations of various historians and historical schools, though instructive and covered here in greater detail than previously, have been examined before by Cyril Black and Boris Kafengauz. It is all the more regrettable then that these chapters were not expanded to include Peter's image as expressed in the writings of poets, dramatists, and essayists. In the absence of this dimension the book takes on more the character of a bibliographic essay than a cultural history. Perhaps a more clearly articulated purpose than the desire merely to convey the evolving image of Peter the Great in Russian thought would have lent greater coherence to the volume.

Even so, Riasanovsky's achievement is impressive. He has covered a vast literature and chosen passages designed to capture the essence of each writer's thought. In the process Riasanovsky is able to substantiate the principal shifts he has marked in Russian perceptions of Peter the Great and his legacy.

DAVID L. RANSEL
Indiana University

JOHN P. LEDONNE. *Ruling Russia: Politics and Administration in the Age of Absolutism, 1762–1796*. (Studies of the Harriman Institute.) Princeton: Princeton University Press. 1984. Pp. xvi, 410. \$40.00.

Scholars wanting to know how the vast Russian empire of the eighteenth century could be ruled from St. Petersburg by an absolute monarch or "autocrat" will find some of the answers to that question in this book. Through exhaustive research in *The Complete Collection of the Laws of the Russian Empire* and many other sources, including archival materials, John P. LeDonne provides a wealth of detailed information on how "the autocracy" collected revenue, settled disputes, instilled obedience, and performed other essential tasks of government. He pays particular attention to the social context of administration and emphasizes the dominant role of the serf-owning nobility.

Strong in its descriptions of administrative practice, the book is notably weak in its explanations of the causes, origins, and motives of those practices. Lacking evidence on those subjects, the author repeatedly insists on inferring a single cause or motive from one of several consequences. A particularly glaring example occurs on page 74, where, having shown that the newly created post of governor-general was soon filled with both supporters and opponents of Potemkin, the author asserts that this evidence "strongly suggests" that the post was a sinecure devised by Potemkin to reward the former and placate the latter. Neither LeDonne nor any of the authors cited in his bibliography, however, provide evidence to show that Potemkin had anything to do with creating the post in question, and a great deal of evidence exists to show that he did not.

The same techniques of argumentation are used to support the author's repeated insistence that the provincial reform of 1775 was introduced to provide jobs for demobilized junior officers or to secure the maximum exploitation of other classes by the nobility or both. Again, he provides no direct evidence to show that such exploitation was greater under the new system than it had been under the old one or that the authors of the reform had intended it to be. LeDonne simply asserts that the reform of 1775 was promulgated by Catherine II for the purpose of delivering the state and economic

peasants into the hands of the local nobles. But if Catherine had wanted to "maximize" the nobles' exploitation of state and economic peasants, as LeDonne claims, one wonders why she repeatedly and steadfastly refused to enserf those peasants to private lords, as many nobles wanted her to do. Instead, it was her son Paul, whom LeDonne portrays as the leader of a bureaucratic, anti-serf owner reaction to the reform of 1775, who gave away hundreds of thousands of state peasants to the serf owners. Similarly, the author baldly asserts that Catherine conducted "a foreign policy designed to open up new lands for the ruling class" (p. 205), yet one looks in vain for any evidence to support such a sweeping and controversial assertion.

In these instances and many others the author's attempts to go beyond the factual evidence to explain the "true meaning" or the "essence" of an institution or an enactment are so contentious and so unconvincingly presented that readers may overlook or discount some of the many valuable things in this book, such as the excellent sections on taxation and finance.

ROBERT E. JONES
University of Massachusetts

E. V. SOBOLEVA. *Organizatsiia nauki v poreformennoi Rossii* [The Organization of Science in Postreform Russia]. Leningrad: Nauka. 1983. Pp. 260. 2 r. 10 k.

This book represents a modest effort to describe the growth of scientific institutions in Russia from 1860 to 1917. Sketchy and descriptive, E. V. Soboleva's study covers three general problems related to the institutional dynamics of Russian science. First, it surveys the major steps in the development of a national science policy and a unified network of science centers. It examines the legal status of scholarly professions, the structure of government committees dealing with matters of scientific scholarship, and the complex problem of financing the national commitment to scientific research. Second, it portrays the growth of the major structural components of the institutional base of science: the St. Petersburg Academy of Sciences, institutions of higher education, learned societies such as the Russian Technical Society and the Free Economic Society, and research laboratories of larger industrial enterprises. The study deals almost exclusively with the specific features of individual sectors and shows little interest in institutional links that made these sectors part of a unified national system of scientific centers. The author has been particularly successful in describing the political, organizational, and financial dilemmas of the St. Petersburg Academy of Sciences and in surveying the rise of modern universities. Third, it reviews governmental efforts to

develop a stable and predictable source of scientific manpower. The author devotes particular attention to the growth of a uniform national system of academic ranks and titles and to the procedures of selecting and training "professorial candidates." Particularly informative is the section dealing with the procedures of selecting students for postgraduate training in Western European universities.

The book has one major shortcoming: it does not deal in concrete detail with the emergence and evolution of a private institutional sector in science. The growth of science philanthropy, a truly remarkable development during the first seventeen years of the twentieth century, deserves much more attention than it receives in this book. The same goes for the appearance of private universities and other institutions of higher education and for the rise of privately funded research centers. The new state-supported technical schools contributed not only to the diffusion of modern industrial techniques but also to the creation of a wider base for theoretical concerns in science. There were strong reasons, for example, why relativity and quantum theories did not come to Russia through the St. Petersburg Academy of Sciences and the universities, the traditional centers of scientific learning, but through the newly founded St. Petersburg Institute of Technology, unfettered by rigid curricular restrictions, vested academic interests, and local scientific traditions. The book does not examine the persistent and dedicated search of the scientific community for a national organization that would protect science as an intellectual endeavor, scientists as a professional group, and academic autonomy as a basic condition for healthy scholarship.

The basic strength of this book is in the description of the institutional dynamics of Russian science during the last decades of the nineteenth century. For reasons that are not stated, the author has made little effort to elucidate twentieth-century developments. Based on an extensive use of archival material, the book offers new insights on an important sector of Russian national life.

ALEXANDER VUCINICH
University of Pennsylvania

G. M. HAMBURG. *Politics of the Russian Nobility, 1881–1905*. New Brunswick, N.J.: Rutgers University Press. 1984. Pp. xii, 296.

G. M. Hamburg has produced an important, useful, and original work that fills a gap in Western scholarship on the Russian nobility. His book describes some of the effects of the worldwide depression on the privileged elements of the Russian agrarian sector and thus complements Terence Emmons's work on the nobility during emancipation and

Roberta Manning's massive study on the nobility in the 1905 revolution. Hamburg is thoroughly familiar with the extensive recent Soviet scholarship on the *dvorianstvo*, which he takes seriously and exploits intelligently. He has also made excellent use of a mass of archival material that demanded considerable patience and technical competence. Although Hamburg strongly focuses on politics, he has produced an economic history of the nobility that greatly enhances our knowledge of the subject.

Hamburg is among those who believe that Russia experienced a severe agrarian crisis at the end of the nineteenth century, and he makes clear that not only the peasantry but also the nobility suffered during those years. In fact, his central argument is that the state was unwilling and unable to help the traditional aristocracy weather the crisis.

Hamburg takes great pains to demonstrate that the traditional *dvorianstvo* had all but disintegrated by 1900. He details the great range of wealth, occupation, nationality, and attitude that made it impossible for the nobility to coalesce as a united social estate. In any rigorous sense, that estate no longer existed by the late nineteenth century. This was one of the essential reasons for the nobility's ultimate political paralysis.

The basis for this paralysis was the *dvorianstvo's* highly ambiguous relationship with the autocracy. The core of Hamburg's book (and the central element of its originality) deals with this problem. In detailed chapters on the Noble Land Bank and on railroad rate policy, Hamburg clearly demonstrates the concrete ways state policy harmed the landed nobility. He takes us through the bureaucratic struggles on these questions and shows the ways the nobility—through personal appeals, petitions of noble assemblies, and *zemstvo* declarations—attempted to change the government's course. What we see is a breakdown of the once-symbiotic relationship between the bureaucracy and the nobility. In order to industrialize, the state could no longer rely on the landed classes, which had proven so ill-equipped to adapt to the world of the market. The nobility felt abandoned, but it could never oppose the government in a principled, consistent, and organized way. This in part resulted from the gentry's own peculiar development but was also caused by the closed political system that operated in Russia before 1905. Hamburg then offers useful comparisons to the American and German responses to the Great Depression. He ultimately concludes that the Russian nobility was most like the French aristocracy before 1789.

One would have liked more attention given to the role of the *zemstvo*, even if it has been covered by other scholars. In addition, the author's attack on what he calls "class analysis" is gratuitous. Neverthe-

less, this is a welcome addition to our knowledge of prerevolutionary Russia.

BOB EDELMAN
University of California,
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ROSE L. GLICKMAN. *Russian Factory Women: Workplace and Society, 1880–1914*. Berkeley and Los Angeles: University of California Press. 1984. Pp. xiii, 325. \$28.50.

Recently published memoirs and monographs have projected an increasingly sharper image of the life and living conditions of Russian workers in the late nineteenth and early twentieth centuries. Factory workers in Russia experienced all of the hardships familiar elsewhere in the early stages of industrialization. But features once obscure in the historical picture are now emerging in clearer light, including the special experience of women in the factories.

Rose L. Glickman's informative study is the first to focus specifically on the Russian female factory worker in the decades leading to the Revolution of 1917. Drawn from a variety of sources, including Soviet archival materials, this carefully researched and balanced work will be of interest to comparativists and students of women's history as well as to Russianists. It offers useful tables, photographs, an extensive bibliography, and an index. The first half of the book describes conditions facing women workers in Russia (in the countryside, in the city, and in general) and changes in the composition and nature of the female work force. The remainder deals with the range of factory women's reactions (social, economic, and political) to the situation confronting them. In the latter section the author reviews some issues explored in other recent investigations, for example, the question of Marxist attitudes toward women and the clash of feminism and socialism.

Glickman points to three intractable problems faced by factory women: the pervasive patriarchal attitude that held all women to be inferior and properly subservient to men; the lower wages paid to women, largely as a result of this attitude; and the sexual exploitation of women in the workplace. The study reveals that Russian workers definitely lacked proletarian solidarity where gender differences were involved. Factory workmen showed little interest in supporting women strikers. In fact, when union leaders harangued their male members to stand by the woman worker in her time of need, their argument, *faut de mieux*, was "Let her stand beside you so that she cannot shoot you in the back" (p. 214).

Repeated objections were raised to the appointment of female factory inspectors, although one-third of all Russian factory workers were women by

1914 and women were reluctant to report sexual abuse to male inspectors. Despite the shortcomings of their working and living conditions and the inadequacy of their wages, few factory women were politically active. Yet Glickman rejects charges that they were politically "backward" and suggests other explanations for their behavioral patterns.

The period under review did witness improvements in the position of some factory women: limitations on night work (a mixed blessing) and some shortening of the work day. But little sense of progress is conveyed. Perhaps the most arresting feature of this study is a table (p. 107) showing the average wages of women in different industries as a percentage of the average wages of men in the same industries in 1914. Depending on the industry, women earned from 40 to 70 percent of men's wages. Since most women worked in textile industries, which were at the high end of the comparative scale, the average female wage appears to have been about two-thirds of the average male wage. Much has changed since 1914, but the average wage of women in the Soviet Union today is still about two-thirds of the average wage of Soviet men.

DOROTHY ATKINSON
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S. A. SMITH. *Red Petrograd: Revolution in the Factories, 1917–1918*. (Soviet and East European Studies.) New York: Cambridge University Press. 1983. Pp. x, 347. \$47.50.

A number of recently published studies testify to the growing richness and sophistication of contemporary Western historical work on late imperial, revolutionary, and early Soviet Russia; S. A. Smith's *Red Petrograd* is among the most valuable of these new works. Smith primarily concentrates on the period from February to October 1917 and considers such important issues as the democratization of the factory order and the development of the Petrograd labor movement after February; the structure, functions, and role of factory committees; the development of trade unionism and wage conflicts; workers' control in theory and practice; and the social structure of the Petrograd labor movement in 1917.

The book is based on exhaustive study of data from Soviet archives, the 1917–18 Petrograd socialist and trade union press, and published documents and memoirs. It builds on a rich body of relevant previously published Soviet and Western secondary studies, and, throughout, Smith does an impressive job of bringing knowledge of European labor history to bear on analysis of developments in Russia. His book provides a wealth of new data and important insights as well as a compelling interpretation of the experience of Petrograd labor in 1917.

Smith's study deepens and, in numerous instances, revises commonly held wisdom about the impact of the revolution on factory administration and life. For example, whereas Western historians have tended to portray the workers' control movement as an anarchist-syndicalist one that sought to oust the bosses and allow the workers to run the factories themselves, Smith shows that the "workers'" control movement arose "spontaneously" among Petrograd workers in the spring of 1917 as a practical response to economic problems. At this stage it was directed toward limiting economic disruption, maintaining production, and preserving jobs. Factory committees became bolshevized in the summer of 1917, a period of deepening economic and political crisis and mass radicalization. As more and more workers became threatened with loss of jobs, the scope of workers' control gradually widened. The previous distinction between oversight and direct management functions disappeared, and the demand for workers' control of production and distribution became linked to the call for transfer of power to the soviets. Nonetheless, even in the aftermath of October actual factory seizures were a rarity in Petrograd and, in most cases, undertaken as a practical measure to keep factories running. Similarly, Smith rejects the common assumption that from the start the Bolsheviks intended to impose strictly centralized controls on the workplace. He indicates, to the contrary, that in October there was little unanimity among Bolshevik leaders on key economic issues and that, at first, even Lenin leaned toward according factory committees considerable power locally. Drastic industrial disintegration, with which factory committees were helpless to deal, ultimately tolled the death-knell for workers' control and the committees.

While concluding that the Bolshevik seizure of power was welcomed by a vast majority of Petrograd workers as the inauguration of revolutionary government by the people, Smith devotes relatively little attention to politics. Hence, readers unfamiliar with the broader political context in which the intricate economic and social struggle he describes was played out may find parts of the analysis difficult and may not comprehend the immensely important, dialectical relationship between the evolving situation and behavior of factory workers and the success of the Bolsheviks in 1917. Also, although Smith's preoccupation with workers in the strategically crucial capital city of Petrograd has enabled him to provide about as comprehensive and meaningful a look at their experience as one could wish for, from his account one learns very little about labor in the rest of revolutionary Russia. Fortunately, Diane Koenker's solid study, *Moscow Workers and the 1917 Revolution* (1981), provides an opportunity for interesting comparisons between

Petrograd and Moscow workers; moreover, important investigations of the situation in provincial cities, either already in press or currently underway, hold out the promise of further broadening the inquiry that Smith has significantly enriched with this masterful study of Petrograd labor.

ALEXANDER RABINOWITCH
Indiana University

HELMUT ALTRICHTER. *Die Bauern von Tver: Vom Leben auf dem russischen Dorfe zwischen Revolution und Kollektivierung*. Munich: R. Oldenbourg. 1984. Pp. 373.

This monograph about the peasantry of the Tver region (northwest of Moscow) is welcome news for students of Russian and Soviet history. It deals with a classically rural area, affected but still untransformed by industrial development and, like many, actually "backward," even in this so-called central industrial region, the most important conglomeration of Russia's factories.

Helmut Altrichter gives us an economic, social, and cultural picture of the peasantry in the Tver region from the 1917 Revolution through the New Economic Policy and ends with a brief statement about collectivization of the peasantry and its aftermath.

In addition to the more usual aspects concerning agricultural production, the role of nonagricultural supplementary earnings, and state institutions in the countryside, Altrichter gives a good overview of "local society" about which less is known and informs the reader about the functioning of the rural community, including classes and cliques, festivals, religion, popular medicine, and magic.

All in all, this is an excellent text for seminars, which will probably also profit from the statistical appendix with eighty-seven pages full of interesting data culled from good local and national sources. The author used an impressive bibliography of primary sources and secondary works, and his craftsmanship is, on the whole, very thorough.

The reviewer welcomes this monograph warmly not only because there are not enough like it but also because the peasantry, even in countries smaller than Russia, exhibits an enormous regional diversity, which is often expressed by the idea that "localism" is a central trait of village societies.

From this point of view the book raises a problem: most of its findings are already well known from the existing Western literature on the subject (not to mention Russian works). This applies to the weakness of Soviet and party institutions and the official ideology in the countryside at that time, to the still-continuing activity of the *mir*, as well as to the different traits of rural society that accounted for its

unpreparedness for a policy such as "collectivization." The reader who knows the subject broadly will enjoy the detailed treatment but will find only a confirmation of what is known and will wonder why it is so. Is it that the peasantry was actually more uniform than its "localistic" traits would suggest? Or would the choice of a different region have unraveled something still unknown or misunderstood?

It is not possible to engage in a discussion of questions like these in a short review. The author does not seem to notice that there is a problem, but, clearly, he approached his topic by asking mostly questions that have already been answered. Perhaps if he had asked some new questions, he could have enhanced the value of his painstaking research.

MOSHE LEWIN

University of Pennsylvania

JURI HOCHMAN. *The Soviet Union and the Failure of Collective Security, 1934–1938*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1984. Pp. 253. \$29.95.

JONATHAN HASLAM. *The Soviet Union and the Struggle for Collective Security in Europe, 1933–39*. New York: St. Martin's. 1984. Pp. xii, 310. \$29.95.

One is reminded when reading these books of the "Communism on the Horizon" slogan whereby "the closer one gets, the further one is away." For neither Jonathan Haslam's meticulous record of Soviet collectivist diplomacy nor Juri Hochman's insightful analysis of the internal and external dimensions of collectivism answers the basic question of why the Soviets pursued a policy that did little to secure them, led ultimately to failure, and can even be considered a charade. They could have made no overtures to the West and achieved the same results with less trouble. Such remain the obfuscations of Soviet foreign policy for historians denied access to necessary archival materials.

In interpreting Soviet collectivist behavior, Haslam basically argues that from 1933 to 1939 the Soviets gave up their traditional policy of exploiting friction and antagonisms within the capitalist camp and chose instead to "struggle" for collective security, "ushering in the most pro-Western period Moscow has ever seen" (p. 1). They did so, Haslam asserts, because "the German Government threatened the peace of Europe and thereby the security of the USSR" (p. 1). Such a thesis is not new and is probably the logical conclusion if only Western diplomatic records are considered and Maxim Litvinov is deemed the sole keeper of Soviet foreign policy.

Haslam traces Litvinov's "struggles" and failures, defending Soviet sincerity all the way. He interprets the Franco-Soviet pact as a long-dreamed-of "prize

for which they were apparently willing to trade revolutionary nationalism in Asia," but, he continues, France "blasted enough holes in" the agreement to deny its utility (p. 27). Likewise, during the Abyssinian crisis the Soviet Union made a staunch defense of the covenant only to find this an irritant rather than an aid in warming up the West. Not surprisingly, Haslam asserts that the Soviets were entirely prepared to come to Czechoslovakia's defense in 1938 but were betrayed by the West's appeasement. Hence, Stalin was driven in 1939 to the policy of "fortress Russia." Then, in an about-face, Haslam explains that the West betrayed Russia because, "however much Mother Russia attempted to conceal the fact, her revolutionary petticoat kept dropping below the hemline of her ill-fashioned dress" (p. 231). Does Haslam mean that the Soviet "struggle" was duplicitous to begin with?

Hochman, by contrast, argues that the Soviet Union joined the Western security system not just to oppose Germany but also to secure themselves East and West. And the failure of the policy cannot be blamed on the West, which could not count on the Soviet Union owing to the problem of Soviet access to Western and Eastern battlefields—a problem the Soviets did little to resolve and much toacerbate. Meanwhile, the purges emasculated potential Soviet military and political help to the West, and it was the Soviet Union, not the West, that fudged on the issue of aid to Czechoslovakia during the crisis of 1938.

What is new in Hochman's book is, perhaps, the suggestion that motivation behind Soviet foreign policy is not the continuity of Russian imperial interests nor the pursuit of revolutionary goals nor both (p. 171) but rather "the concern of the elite to maintain power" (p. 172). In explaining how internal insecurity drove the external policy, Hochman stresses that Stalin continued attempts to get an agreement with Germany and that the French connection posed unwanted risk and imposed a certain restraint on methods of governing the Soviet Union and approaches to neighbors. Likewise, the united front was a tactic to maintain the survival of the main Soviet sections in Europe, endangered by pressures against the Comintern whose growing involvement in Spain smacked of the Trotskyite internationalism so politically dangerous to Stalin domestically. Meanwhile, the purges went on unfettered by their external impact on either Germany or the Allies. In his view Soviet foreign-policy decisions have always been mainly exigent and short-term in nature (p. 90) and feudal in outlook (viewing foreign states as either vassals or enemies). Ultimately, Hochman argues that for the Soviet Union the way to prepare for war is to avoid revolution.

For a trek through collectivist diplomatic records and an occasional clever turn of the phrase,

Haslam's is the book to read. But for a comprehensive and thought-provoking examination of the linkage between internal and external policy Hochman's substantial contribution is the better choice. These books are probably not the last word on collectivism, however. Before this issue can be put to rest, the question of national aspirations (as opposed to defense), the impact of the communist ideological dynamic, internal bureaucratic decision making and debate, personalities, and perhaps the continuing mystique of Russian involvement in European affairs have yet to be explored within the context of the Soviet collectivist activities of the 1930s.

MARILYNN GIROUX HITCHENS
University of Denver

JOACHIM HOFFMANN. *Die Geschichte der Vlassow-Armee*. (Einzelschriften zur Militärischen Geschichte des Zweiten Weltkrieges, number 27.) Freiburg: Rombach. 1984. Pp. 468, xviii. DM 24.

The "Vlasov Army" of this intensively researched monograph was the Russian Liberation Army, the ROA (*Russkaia Osvoboditel'naia Armia*), founded in German-held Prague in November 1944 by a captured Soviet general, Andrei Vlasov, with the approval of Heinrich Himmler, the chief of the German SS. The ROA saw exactly three days' action in World War II: two against the Soviet army on the Oder River front and one against German army and SS units in Prague. It never had more than one combat-ready division, and that not until late January 1945, and its total strength of fifty-six thousand men constituted a minor fraction of the roughly two million former Soviet soldiers who served with the German forces in the war. Knowing the futility of partnership with Germany, Vlasov and his officers pursued a dream of an American alliance and managed thereby only to implicate the United States in the nightmare of Soviet retribution that befell themselves and their troops.

Joachim Hoffmann, a member of the *Bundeswehr's* Military History Research Office, has in the past written extensively on the *Freiwilligenverbände*, the military contingents drawn from Soviet minorities that served in the German forces during World War II. He has now mustered a massive array of evidence on the ROA, which claimed separate status as a cobelligerent. His purpose is not just to relate the story of the ROA, which has been done several times already, but also to determine and document its historical significance. This he sees as depending much more on what the ROA was—namely, the culmination of the ethnic-Russian and Russian-nationalist Vlasov movement—than on the ROA's military accomplishments. He recommends his work on this "highly explosive theme" to Soviet

scholars, whose government has suppressed all open discussion of anything having to do with Vlasov and among whom even such outspoken dissidents as Alexandr Solzhenitsyn, Roy Medvedev, and Alexandr Nekrich have disparaged the so-called Vlasovites.

Since the official silence has been complete, the case against the ROA is, as Solzhenitsyn put it in volume 1 of *The Gulag Archipelago*, that there was no "real Russian Liberation Army"; the men who wore the ROA insignia, a blue cross of St. Andrew on a white field, were merely pawns forced to fight for the Germans to escape certain death in the prisoner-of-war camps; and Vlasov and his generals "had nothing to do but play cards in Dahlemdorf, near Berlin" (pp. 255–61). Hoffmann offers an altogether different picture of the men, the leadership, and the nature of the ROA. He finds survival or any other form of compulsion not to have influenced enlistment. He shows that conditions in the prisoner-of-war camps improved before the Vlasov movement made its appearance in early 1943 and that the ROA always had more volunteers than it could absorb, even though the men knew what would happen to them if they fell into Soviet hands. The officers, he maintains, were—like Vlasov, who had been celebrated in the Soviet press as an exemplary division commander before the war and as a hero of the battles of Kiev and Moscow in 1941—some of the best military professionals the Soviet armed forces had produced and were neither unprincipled traitors nor feckless German puppets but men dedicated to the cause of the Russian nation.

Hoffmann concludes, therefore, that the ROA, in spite of appearances to the contrary, was the Russian liberation army it purported to be. Motives, however, are exceedingly difficult to document, and on some points one can question whether the author has actually done that. Nevertheless, he has, as he says, demonstrated "the Soviet Army's political and moral assailability" in World War II.

EARL F. ZIEMKE
University of Georgia

VICTOR ZASLAVSKY and ROBERT J. BRYM. *Soviet-Jewish Emigration and Soviet Nationality Policy*. New York: St. Martin's. 1983. Pp. vii, 185. \$22.50.

Il'ia Il'f and Evgenii Petrov, popular Soviet authors of the 1930s, once quipped that life abroad for a Soviet citizen is about as unreal as life after death. Boris Khazanov, a prominent emigre author, remarked that his generation perceived leaving the Soviet Union as impossible as it is to throw a stone so high that it will never return to earth. And yet in one remarkable decade, between 1971 and 1980, the impossible happened and the unreal was realized.

About a quarter of a million Soviet citizens, mostly of Jewish but also of Russian, Armenian, and German background, were let go.

How has this extraordinary window of opportunity been opened? What are the historical roots of the "Soviet-Jewish anomaly"? What were the basic causes of Soviet emigration? How did the motivations of its participants interact with the intentions of political actors involved? And, finally, what are the consequences for the USSR likely to be? These questions have already produced significant literature in the West, and Victor Zaslavsky and Robert J. Brym formulate their task not so much in terms of describing the phenomenon of Soviet emigration itself but rather in those of evaluating the descriptions of others.

According to the authors, there are two basic schools of thought in the literature on the major forces shaping the Soviet emigration. One, which they call "extrinsic," locates the major forces outside of the USSR; the other, or "intrinsic," locates them within the country and stresses, for example, "the attempt on the part of Soviet authorities to streamline the country's enormous scientific bureaucracy, cut down on the size of its overexpanded new middle class and relieve somewhat the severe urban housing crisis" (p. 4). The authors analyze most scrupulously the arguments of both sides and conclude with a reasonable, if somewhat predictable, answer that each side is partly correct as well as partly wrong, that only a combination of both schools of thought freed of their extremes explains the phenomenon adequately.

The most contradictory part of the book is its concluding chapter on the consequences of mass emigration. Here the authors abandon the reconciliatory stance they maintained in all the preceding chapters and strongly favor one of the contesting sides. As a result, they finally enter "into some pretty murky political waters" (p. 1). They protest against the "extrinsic" notion that escape is the sole avenue of Jewish survival in the USSR. They argue that "democratization [of the country] is a real possibility, heightened, paradoxically enough, by such forces, as the emigration movement itself" (p. 137).

Indeed, if we revise popular assumptions about the nature of the post-totalitarian state and theoretically recognize that Soviet elites are fundamentally divided on a number of basic political issues, then liberalization and reform in the USSR (of the kind that occurred in Hungary and is occurring in China) appear to be real possibilities. The authors, however, are very far from such theoretical revisionism. In fact, they give every indication that they belong to the opposite school of thought. They do not challenge the notion that the post-Stalin USSR remains a totalitarian state in which a monolithic "ruling class has been formed" whose sole purpose is to maxi-

mize its political control over the society. Why on earth would such a totalitarian and united ruling class go for liberalization, let alone democratization? Thus, the theoretical premises of the authors appear to contradict their own conclusions.

By no means, however, does this theoretical inconsistency diminish the practical value of this book as the most comprehensive critical survey of Western literature on the subject.

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University of Michigan

NEAR EAST

F. ROBERT HUNTER. *Egypt under the Khedives, 1805-1879: From Household Government to Modern Bureaucracy*. Pittsburgh: University of Pittsburgh Press. 1984. Pp. xv, 283.

F. Robert Hunter describes the rise and fall of viceregal absolutism in Egypt. This was a system of personal rule—the domination of the wali or khedive—supported by the creation of a new hierarchical bureaucracy and army dependent on the ruler for favor. The author ably shows that within seven decades, from 1805 to 1879, this system collapsed as a result of changes in Egypt's political and administrative order. These changes were wrought by alterations in the country's material infrastructure and its growing dependence on outside assistance in efforts to "develop" the country and enmesh it in the European capitalist system. When key members of the elite established ties outside viceregal households, thereby weakening their loyalty to the ruler, destabilizing the polity, and ensuring the establishment of European control through collaboration, viceregal authority was displaced in favor of imperialism.

The author traces the rise of bureaucrats of Turkish, Egyptian, and other descent as an elite that played a role in dismantling viceregal absolutism. Biographies of the elite, compiled from archival sources, form the backbone of the work, showing the Egyptianization of the administration and the growth of large latifundia.

Since the work depends in part on reinterpreting secondary sources, a number of minor remarks may be in order. To say that peasant flight in consequence of excessive government demands for revenue "emptied entire villages" (p. 31) under Muhammad Ali is an inaccuracy when the author shows more excessive demands being made under Ismail. Muhammad Ali did not "plan another war" in 1837 (p. 31); that was precipitated by the Ottomans. Abbas must be credited with establishing the first railway line in Egypt, although Said expanded it (p.

40). Slaves were not purchased in Constantinople (p. 100) because of an Ottoman embargo against the sale of slaves to Egypt in 1820. Slaves were purchased in Cairo because of the Morean campaign. Male slaves were not rare (p. 102); even village chiefs acquired them during that period. Waqf documents of the late nineteenth century list manumitted slaves.

One should add a word of warning regarding Hekekyan's observations of the Egyptian scene, which were biased and often paranoid. His belief that Egypt would do better under European rule was echoed by no other official and was motivated by spite and lack of identity with Egypt and the Egyptians.

AFAF LUTFI AL-SAYYID MARSOT
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WILLIAM OCHSENWALD. *Religion, Society, and the State in Arabia: The Hijaz under Ottoman Control, 1840–1908*. Columbus: Ohio State University Press. 1984. Pp. xiii, 241. \$20.00.

This important monograph marks a substantial step forward in the study of Ottoman society. William Ochsenwald has made careful and accurate use of Ottoman archival documents in the Başbakanlık Arşivi, Istanbul, as well as supplementary material from British and French archives, to provide a wealth of information on the religion, education, commerce, politics, and social organization of nineteenth-century Arabia on a level and to a depth surpassing anything previously available in English on any province outside the central districts of the empire.

Ochsenwald shows that Arabia's paucity of natural resources, combined with an urban population composed mainly of outsiders concentrating on religious pursuits, left it dependent on Egypt and Constantinople for sustenance and support. He demonstrates how the annual pilgrimage of Muslims from around the world constituted the basic link between religion and society, making an important contribution to the area's economy but at the same time creating major problems of public health that were dealt with only in part. In his discussions of business and justice in particular, one sees once again the efforts of foreign residents to rely on the capitulations, not merely to gain financial advantage over local Muslim merchants but also to satisfy long-standing prejudices against Muslims, Islam, and Islamic institutions. Ochsenwald outlines the important role played by the guilds, and he provides much information on slavery and the slave trade, which were considered natural and necessary parts of life. He traces the fluctuating relationships be-

tween the Hashemite *amirs* representing the local nobility, and the Ottoman *valis*, or governors, whose mutual desire for power dictated cooperation despite differing interests. He traces the circumstances that led to occasional massacres of foreigners during the nineteenth century, caused not simply by "Muslim fanaticism," an image commonly accepted in the West, but rather by long-standing economic rivalries as well as religious hostility on both sides. His examination of Arabia's population and society adds depth and breadth to the pioneering demographic studies of Justin McCarthy, also based on Ottoman archival records.

Ochsenwald is to be congratulated for his systematic use of some of the more difficult and inaccessible Ottoman archival materials. In every respect he provides substantially more useful details out of Ottoman archival sources than those provided previously by Western scholars on the basis of European travel reports, which can now be seen, in the light of modern research, to be of very limited value. The study demonstrates once again the futility of relying on non-Ottoman sources alone for study of conditions within the Ottoman empire, particularly in the Balkan and Arab provinces.

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DONALD QUATAERT. *Social Disintegration and Popular Resistance in the Ottoman Empire, 1881–1908: Reactions to European Penetration*. (New York University Studies in Near Eastern Civilization, number 9.) New York: New York University Press. 1983. Pp. xxi, 205. \$35.00.

Donald Quataert asserts that the main cause of the Young Turk revolution "was the impact of Western economic expansion" (p. xv). He uses the case-study approach "to examine the impact of the Western economy on Ottoman rural and urban dwellers . . . and to ascertain the variety of ways Ottoman subjects interacted with that intruding force" (p. 4). His book consists of an introduction, a chapter on "The Nineteenth-Century Ottoman Economy," five case studies, and a conclusion. The case studies, focusing on Istanbul and Anatolia between 1881 and 1908, are extensively documented, and an impressive list of sources is appended. Quataert's presentation is lucid and holds the reader's interest.

In 1884 the Régie monopoly brought domestic Ottoman tobacco production and consumption under foreign control. Popular reaction was expressed primarily by smuggling, and the government refused to cooperate with the Régie in suppressing it. Unable or unwilling to cancel the concession or to

halt the contraband traffic, the government lost popular support and its authority was undermined.

Exploitation of the Ereğli coal mines in Anatolia was undertaken by a French company in 1896. The state hoped thereby to ensure an adequate domestic supply of coal for military and strategic purposes and to increase tax revenues while maintaining a stable agrarian population by employing in the mines part-time, rotational workers, who were recruited from adjacent farm villages. In these respects the enterprise was successful, but another segment of the Ottoman economy and population was opened to the penetration and influence of European capital. Labor unrest and strikes, which revealed the conflicting views of the Ottoman central government and those of its local officials and subjects, were among the factors contributing to the financial failure of the enterprise.

The German-financed Anatolian Railway Company concession of 1899 was granted by the Ottoman government to construct a railroad largely for military and strategic purposes, but it affected agricultural production and marketing patterns and created jobs that produced a new class of workers. Quataert studies the composition of the labor force, the formation of a union in 1908, and the strike soon after. Clashing interests of the "moderate" union leadership, dominated by European and Ottoman Christians from the managerial levels, and the "more radical" Muslim Turkish work force "exacerbated the continuing development of ethnic rivalries . . . and promoted the rise of Turkish nationalism" (p. 91).

Efforts to develop modern, efficient port facilities at Istanbul and to revitalize the capital's commercial life produced a struggle among the French-controlled Istanbul Quay Company, the port workers guild, and the Ottoman government. Pressure from merchants and investors obliged Abdul Hamid's regime to back the concessionaire, thus alienating a powerful and cohesive group of workers and providing additional support to the Young Turks. The guilds, stubbornly resisting European penetration and successfully protecting their members' jobs, emerged weakened but still a force to be reckoned with.

The port workers' guilds in alliance with the Young Turk elite were crucial also to the success of the Ottoman boycott of Austro-Hungarian goods and shipping following the Habsburg annexation of Bosnia and Herzegovina (1908). The boycott brought to the surface the different economic interests of the Muslim and non-Muslim communities, adding to tensions within Ottoman society and virtually assuring the subsequent displacement of Christians from Turkish commerce and industry.

Quataert has given us a rare glimpse of how some ordinary Ottomans—tobacco farmers, miners, rail-

road workers, dock hands, and merchants—resisted European economic penetration while others accommodated themselves to it and benefited from it. In so doing, he has provided convincing evidence that the impact of Western economic expansion contributed significantly to the fall of the Hamidian regime, "a victim of social and political change it had failed to control" (p. 154).

RICHARD L. CHAMBERS
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KENNETH W. STEIN. *The Land Question in Palestine, 1917–1939*. Chapel Hill: University of North Carolina Press. 1984. Pp. xviii, 314. \$29.00.

Land has always been central to the Israeli-Palestinian struggle. The purchase of land in Palestine by Zionist settlement agencies during the British mandate enabled the Zionist movement to create the territorial nucleus of a Jewish state and was facilitated by the Jewish Agency's special legal status and the Zionist leaders' close personal ties with British politicians. The Zionists' efforts to prevent laws from being passed that would protect Arab landholdings or would ban land sale were overwhelmingly successful until 1940, when the first and only restrictive legislation came into force.

Using the massive land records contained in Israeli archives, Kenneth W. Stein examines in detail two aspects of Zionist land policy in Palestine: purchase of land and influence over British policy. He concludes that the Jewish community was able to purchase the core of a national territory by 1939. Since the Jewish Agency extraterritorialized all the land that it purchased—banning not only resale of the land to Arabs but also the hiring of Arabs to work it—the Arab community's control over its political destiny eroded with each acre sold.

In the 1920s non-Palestinian landlords living in Lebanon or Egypt were the main land sellers, but by the 1930s several prominent Palestinians sold tracts. By the mid-1930s even small landowners were selling parcels.

Stein recognizes that most land sales were caused by economic necessity, notably, heavy indebtedness and crop losses resulting from drought or infestations of locusts or mice. Moreover, he concedes that "there was little that a generally poor, unorganized, uneducated, unsophisticated and splintered Arab community could do to defend itself" against Jewish encroachment (p. 217). Nevertheless, he maintains that "Palestinian Arab land sales meant the absence of true commitment to Palestinian nationalism" (p. 70). This is an oversimplification. Even though several prominent Arab politicians (listed in appendix 3) did sell land, thus contradicting their professed nationalist ideas, this does not mean that the Pales-

tinians were on the whole hypocritical or non-nationalistic. Stein's own description of the Arabs' impoverishment, lengthy revolt, and attempts to pressure the British government to pass legislation that would ban land sales underlines the Arabs' "despondent and frustrated feelings" (p. 146) at their inability to stem the Zionist movement. Stein admits that the Arabs only sold 7.6 percent of the land surface of Palestine; the rest was won later by Israel through military conquest.

Stein's examination of the Zionist leaders' ties to British politicians and their remarkable influence over British policy making is refreshingly candid. He concludes: "A major reason for the ultimate success of the founding of the Jewish National Home was the Zionists' ability to influence policy-making for Palestine. Zionist officials helped draft the Balfour Declaration, the Palestine Mandate, ordinances and legislation . . . ; influence was exercised over the appointment of individuals and officials who played pivotal roles in Britain's design and management of the Mandate" (p. 80). In these ways restrictive legislation was "derailed" (p. 81), and Arab efforts to exert counterpressure were easily deflected.

Overall, Stein has written an important study, relying on unique documentation. It is weakened, however, by his superficial and simplistic categorization of Palestinian nationalism. Moreover, the study would have been strengthened significantly if Zionist land policy had been examined in the context of the simultaneous effort to build the Jewish nation in Palestine through promotion of mass immigration, establishment of a separate Hebrew-language educational system, creation of a semiclandestine military force, and institutionalization of a governmental structure that could assume control when independence was proclaimed. Land purchase was part of this comprehensive effort. Given the limited amount of land that was actually purchased, however, landholdings alone could not have enabled the Zionist movement to establish a state in 1948.

ANN M. LESCH
Universities Field Staff International

AFRICA

EUGENIA W. HERBERT. *Red Gold of Africa: Copper in Precolonial History and Culture*. Madison: University of Wisconsin Press. 1984. Pp. xxiii, 413. \$32.50.

The dust jacket hails this impressive piece of research as the "first cultural history of a metal," and in general this is an apt description. Eugenia W. Herbert addresses the subject from a multidisciplinary

perspective, examining technology, history, oral tradition, economics, symbolic anthropology, and archaeology. The volume is comprised of three sections that consider, in turn, the technical aspects of copper metallurgy (ore sources, mining, smelting, smithing, casting, and alloying), trade in copper before and after European contact, and the role of copper and brass in traditional African societies.

In Africa copper was a medium of exchange as well as of art, and it was also a symbol of wealth, power, and well-being. In contrast to the European and Islamic worlds, it was valued far more than gold, a metal seldom as assiduously exploited, and then mainly for export to the bullion-hungry markets of Eurasia. Copper, however, was mined mostly for internal use, a good deal of it being subsequently imported as well. Much more is known about these imports and the commercial networks related to them than is known about the precolonial trade in Africa itself. The copper of Katanga, for example, clearly was being disseminated to other parts of Zaire and even to the Interlacustrine kingdoms of East Africa by the late eighteenth and early nineteenth centuries, but the structuring of that trade is poorly understood. The metal may have been a standard of exchange, but its role in that respect is clouded by the high prestige value it enjoyed. Although ingots were produced in areas like Central and Southern Africa, the presence of many such examples in graves could be indicative of the metal's more important function as a status symbol than as a kind of currency. From European archives it is easier to estimate the quantity of copper and brass brought to African markets than it is to calculate the amount of copper produced locally. But by any type of quantification it seems certain that more of the metal entered the regional economies than has ever been recovered archaeologically.

In addition to hammered or cast art works of pure metal—among which those of Ife, Benin, and Igbo Ukwu perhaps stand supreme—copper and brass were employed to embellish wood carvings and other ritual objects as well as utilitarian ones. In some groups the same materials were used for bodily ornament, sometimes worn in prodigious quantities to reflect status and wealth. The properties of these metals, whose qualities were deeply imbedded in myth and tradition, were widely associated with political power, fertility, life stages, and propitiation of the ancestors.

As the author states in her final paragraph, "economic history is not simply concerned with material needs; it is inseparable from the values people live by." That, in essence, is what the book is all about, although it does not skimp on practical details either. *Red Gold of Africa* provides a fine resource for

anyone interested in the technology, economy, and social history of precolonial and postcontact Africa.

CREIGHTON GABEL
Boston University

This team has done a good job in gathering data and presenting an account of the social dynamism of a small African metropolis.

DANIEL F. MCCALL
Boston University

ERNESTA CERULLI and GIOVANNA PARODI DA PASSANO.
Grand Bassam anni '80: Dinamica sociale di una metropoli in miniatura. (Paperbacks Università.) Ivrea: Hérodote. 1984. Pp. xiii, 215. L 24,000.

A town of about thirty-five thousand inhabitants that was a former administrative center in the period of French colonial rule is appropriately termed a "miniature metropolis." Located in the southeastern corner of the Ivory Coast, Grand Bassam is not an old town, even by the standard of the Gulf of Guinea coast; Elmina, Accra, and Benin, for example, were established before Europeans navigated this region. Settled early in the eighteenth century and probably in response to French maritime commercial activity, Grand Bassam became part of a French protectorate in the mid-nineteenth century and reached its apogee around the end of the century. It has since been eclipsed by Abidjan, the present capital.

The population is ethnically mixed but predominantly Akan. This study is partly an ethnography and partly a survey in urban anthropology. Ernesta Cerulli and Giovanna Parodi da Passano compare the local Akan with the better-known Akan peoples, such as the Ashanti and Baule; the local Abure until now had been less extensively described on the basis of anthropological fieldwork. The practices associated with marriage, birth, naming of children, descent system, funerals, magic, and religious beliefs are given in plentiful detail. The economic basis of the life of the townspeople, still partly agricultural, is also presented. A brief "ethnohistory" of the Abure, setting them into a wide historical overview of their West African hinterland, preceeds the sociological sections, and an essay on methodology follows them. The authors argue that the Third World needs a methodology suited to its peculiar conditions. They call for team research in order to bring together the many aspects of economy, social organization, religion, art, politics, and history. The days of a Malinowski, who worked alone (and founded the tradition of fieldwork), are over, they say.

The term "social dynamic" in their title indicates a concern with processes, change, and a time frame. Their focus is on town life (and the quality of life) in an African locality since independence from the former French empire. Thus, they look at wealth, prestige, and power in a society that has *évolué*s (Western-educated) as well as traditional families.

ASIA AND THE EAST

GILBERT ROZMAN. *Population and Marketing Settlements in Ch'ing China.* New York: Cambridge University Press. 1982. Pp. ix, 154. \$34.50.

Gilbert Rozman's case studies of north China's local population data and rural marketing data of the past three centuries should attract the attention of both historians and specialists of contemporary China. In an attempt to define the impact of commercialization and urbanization on the rural population, Rozman presents the ratios of population to marketing settlement (P/M) for the twenty-seven prefectural units in the provinces of Shantung and Chihli (lately known as Hopei). The ratios were computed on the basis of the quantitative data of each of these prefectures for many of the five consecutive periods that divided up the 300 years from the seventeenth to the mid-twentieth century. The subsequent comparisons of the P/M ratio changes among the prefectures suggest a picture of the macroregional distribution of the differential patterns of P/M value changes during the five periods.

Interesting and provocative as they are, these findings do not provide the evidence to support the author in his attempted challenge against the model proposed by G. William Skinner on the changes of rural marketing structure (often labeled as the "intensification cycle"), which is essentially based on the central-place theory as advanced by German economic geographers half a century ago. The various P/M patterns, as ascertained by Rozman, appear generally to confirm, rather than to deviate from, the major phases of changes as predicted by the intensification cycle, especially when the specific findings are restored in the perspective of the microeconomic region where each of the prefectures was situated. Take, for example, the jump of the P/M value in the rural Tengchow prefecture during the early twentieth century, which appears not so "unexplainable" (p. 116) or "difficult to characterize" (p. 121) once we focus our attention on the poor conditions for inland transportation in this hilly region at the northeastern corner of Shantung peninsula. Similarly, the waterway conditions and the lack of motor roads in the rural counties of T'ien-chin prefecture shed light on its P/M values, which are presented in the book as a major chal-

lenge to the Skinnerian model. Of the prefecture's seven component counties, six were situated in the hinterland region that stretched to the south of Tientsin port along the ancient Grand Canal, which was unnavigable in modern times. Rather than provide the hard statistical evidence against the model, Rozman's case studies point to the need for a greater differentiation among the hinterland regions of the treaty ports in modern China.

Any appraisal of such case studies must be weighed against the immensity of challenges posed by the nature and the quality of the quantitative data from China. The lack of access of field work for most of the past thirty-five years has turned demographers and social scientists to comb the historical records for statistical information. Such information is abundantly rich, but researchers encounter difficulties in two aspects. First, the researchers outside China often have no access to some of the crucial sources. For the seven component counties of Lai-chou prefecture, for instance, the author evidently had no access to the marketing data of six of the counties for the Republican period, or the preceding Kuang-hsu (the late Ch'ing) period, or both. A significant test of the Skinnerian model could be made against the corridor area between the national capital city of Peking and the great northern port of Tientsin. But the large number of corridor counties that fell in the Shun-t'ien prefecture provide the marketing data of both crucial periods only for one county (Liang-hsiang). Understandably, the author had to use estimates as substitutes for the missing data. Second, the available data are presented usually in forms unsuitable for the standard treatment of modern disciplines. Rather than actual census figures, for example, the documentary sources from late imperial China usually contained summaries of figures, if not estimates or tax quotas. Finally, there are the ubiquitous traps of over- and underreporting from provincial and subprovincial units, which acted to suit their fiscal or other administrative purposes.

To establish their validity, therefore, the author uniformly tested the quantitative data he had combed from the Chinese local gazetteers against a set of widely used social indicators, including not only the above-mentioned P/M ratio but also the mean household size, the sex ratio (for children and adults), and the percentage of total population under the age of fourteen. His efforts to sift and evaluate such data could occasionally be tedious. But a patient reader will be rewarded with precious glimpses of Chinese social structure at the local level when he encounters the maps and charts that present the interweaving of eleven social groups (occupational/status groups) with twenty local settlement groups (of the walled city and the nineteen suburban and rural units) of T'ien-chin county.

Such findings may, also, serve as the baseline for any study of the urban changes in the county. In view of the incipient trend of granting Chinese scholars access to census data from recent decades, some of the research methods devised by Rozman could be edifying to Chinese researchers as well.

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JOHN E. WILLS, JR. *Embassies and Illusions: Dutch and Portuguese Envoys to K'ang-hsi, 1666-1687.* (Harvard East Asian Monographs, number 113.) Cambridge: Council on East Asian Studies; distributed by Harvard University Press, Cambridge. 1984. Pp. xi, 303. \$20.00.

Drawn mainly from seventeenth-century Dutch and Portuguese records and publications, the core of this book by John E. Wills, Jr., is four chapters describing four embassies that appeared at audiences in Peking before the young Manchu emperor between 1667 and 1686.

The Dutch East India Company in Batavia organized a mission in 1666 after being repeatedly informed that the Dutch could not trade at Foochow unless they presented tribute to the recently enthroned emperor in Peking. The ambassador, Pieter van Hoorn, was instructed to seek "free trade" in recognition of the Dutch tribute and for the action of Dutch ships against "pirates." The fine presents and obeisances were accepted, and the embassy returned to Foochow with a sealed order, unread and not accurately understood once translated, to present tribute every eight years and not to trade otherwise.

Macao, the tiny outpost of the Portuguese empire, was more than a century old and barely viable in 1662 when Ch'ing officials applied a ban on coastal shipping and ordered the evacuation of Chinese residents from the walled enclave. Suffering extremes of starvation and extortion, residents of Macao in 1666 sent a plea to Goa for help. Manuel de Saldanha volunteered to be ambassador, a loan to fund the embassy was sought from the king of Siam, and some tacky presents were assembled in Macao. Ill and short of silver, Saldanha waited in Canton from August 1667 until January 1670, when he finally was allowed to set out for Peking after having agreed not to attempt to discuss formally the plight of Macao. Jesuits at Peking put in more than a word or two on Saldanha's behalf, but Macao's prospects were not strengthened.

Bento Pereira de Faria, who had taken over the returning delegation after Saldanha's death in 1670, went to Goa in 1672 and asked the Portuguese viceroy for a lion, which arrived from Mozambique

in the fall of 1676. Peking was notified, and in March of 1678 the lion, which was the only gift Ambassador Pereira de Faria had to offer, set out for the north. The emperor was pleased with the lion and possibly receptive to the Peking Jesuits' intercessions. After Peking authorities took direct control at Canton in 1680, Macao was informed that its maritime trade was legalized, a privilege the Portuguese alone enjoyed until 1684.

A Dutch ship was allowed to sell its cargo at Amoy in 1684, but the Dutch understood they could not trade again unless they sent an overdue embassy. Accordingly, Batavia in 1685 sent Vincent Paats with another request for "free trade." Instead, Paats left Peking in September 1686 with an imperial decision that the Dutch were to send tribute every five years and could trade in either Fukien or Kwangtung. It was more than a century before another embassy concerned with maritime trade returned to Peking, for the imperial decision had been made to send commissioners to open maritime trade to all comers and regulate it on behalf of imperial rather than provincial interests.

The four embassies were real enough, but who had the "illusions"? Wills gives repeated instances of Dutch, Portuguese, and various Chinese hoping for what could not be and pretending X was Y. The biggest illusion that Wills dispels is the twentieth-century assumption that "the tribute system" was the sole conceptual and institutional framework for the conduct of relations involving representatives of European nations arriving by sea in the seventeenth century. He shows that "the tribute system" was a decoration for court audiences, an occasion for historiographical and poetic conceits, and a bureaucratic expedient for dealing with foreigners at delicate moments when provincial and court politics were in flux. It was disregarded at a word from the Manchu monarch, who seemed to have fewer illusions than anyone else about strangers from afar offering tribute.

This well-written book, along with Wills's earlier *Pepper, Guns, and Parleys: The Dutch East India Company and China, 1662-1681*, is an important contribution to the evolution of a new understanding of "foreign relations" as they were practiced in the later imperial period.

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HOH-CHEUNG MUI and LORNA H. MUI. *The Management of Monopoly: A Study of the East India Company's Conduct of Its Tea Trade, 1784-1833*. (Asian Studies Monographs, number 5.) Vancouver, Canada: University of British Columbia Press. 1984. Pp. xiv, 192.

This book forms a useful addition to the historiography of the East India Company. Hoh-Cheung Mui and Lorna H. Mui have chosen the period from the passing of the Commutation Act in 1784 to the end of the company's monopoly in the Chinese tea trade to Britain. Within this period the authors have given a clear and authoritative account of the policy of the company's directors, their efforts to meet the challenge of the free traders, and the organization of tea purchasing in China. The study also provides an excellent description of the methods of tea production in China, the varieties of tea produced, and the market for each in Britain. This is a good example of business history and takes the account given by H. B. Morse in his *Chronicles of the East India Company Trading to China, 1635-1834* (1926-29) further by the use of new source material.

There is, however, one area in which more information is needed and about which the directors may have had little to say: the relations between the company's officials in China and the Chinese merchants, especially during the period of the revolutionary and Napoleonic wars. The old European companies were in decline, and newcomers arrived from a variety of European states to buy tea. But, unlike the British, the newcomers were not the official representatives of a well-established and prestigious company that, as the authors show, had enormous purchasing power. Following the Peace of Amiens, the French and Dutch, who had been excluded by the war, were back, though only for a time. The wars also presented the Americans with new opportunities, and they entered the China trade in large numbers: there were twenty-two American ships anchored at Wampou in November 1802 when the French Indiaman *Diane* arrived.

Another aspect that needs more attention is the purchase of the tea in China. The Chinese scorned most European manufactured items except printed cotton cloth. Silver, which played a significant role, was obtained in part in London and in part in India. But much of it originated in the Spanish colonies in South America, which were torn by civil war in the early nineteenth century. How did this affect supplies of silver? Was the company able to increase its sales of other commodities from India? The company could hardly deal extensively in bills of exchange—the only alternative.

These criticisms do not in any way detract from an excellent book, which is thoughtful and informative. The authors are also to be admired for their courage in linking the management of the tea monopoly with the current dispute in Britain over the management of state monopolies in coal mining and rail transport. Their fair-minded defense of monopoly should ensure that this book will be read by more

than students of the history of the East India Company, for whom the book is primarily intended.

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WILLIAM T. ROWE. *Hankow: Commerce and Society in a Chinese City, 1796–1889*. Stanford: Stanford University Press. 1984. Pp. viii, 436. \$39.50.

Only recently have economic historians abandoned Max Weber's vision of the Chinese city as hardly more than an overgrown village, dominated from below by archaic extended families and from above by a sophisticated but equally archaic mandarin, and applied to China Weber's contrasting vision of the European city as a merchant capitalist-dominated, autonomous community—the nursery of the Industrial Revolution.

Evidence eventually surfaced that treaty-port Chinese businessmen quickly absorbed new ways from Western businessmen, particularly in Shanghai. More subtle effects exerted by the West over a much wider semicolonized hinterland in places like Shantung and southern Manchuria were noticed next.

Now William T. Rowe's account of nineteenth-century Hankow provides considerable new evidence for what patriotic Chinese, including nativist Marxists like the late Chairman Mao, have long believed: that, even without European goading, some Chinese cities lagged only a century or so behind many European cities in becoming effectively autonomous communities governed by a commercial bourgeoisie; hence, the cities were fit receptacles for the European-style industrial capitalism that finally arrived by the end of the century.

By 1800 Hankow had become the great commercial "hub" of China. From it radiated riverine and road "spokes" to all the major regions of China and East Central Asia and, after 1842, via Shanghai to the rest of the world. Within three centuries of its appearance during the late Ming period, Hankow had become one of the world's twelve largest cities—a somewhat unexpected datum since a leading early reviser of the Weberian view, Mark Elvin, suggested in his pioneering economic history, *The Pattern of the Chinese Past* (1973), that big cities stopped appearing in China after Sung times.

After a vivid first chapter on the city's founding and its geographic setting, Rowe demonstrates in successive chapters how the burgeoning of the salt, tea, and banking trades assembled in Hankow a numerous and prosperous bourgeoisie from all over China. He also shows that this bourgeoisie's guilds gradually transcended the limits of local origins and particular trades to combine into imposing confederations that exerted legitimate dominance over

municipal government and nourished economic and legal institutions that would carry the city toward a genuine industrial "takeoff" before the end of the nineteenth century.

Rowe has crafted a solid piece of scholarship, using all the relevant Japanese and Chinese secondary works (although freeing them of their Weberian and Maoist-Marxist matrixes) as well as an enormous volume of primary source material, both Chinese and European. He and his fellow revisers of Weber have now made it impossible for us to blame the peculiarities of twentieth-century China's hobbled industrialization on some sort of "Oriental despotism" distortion of her pre-twentieth-century cities, thereby clearing the way for some new explanation.

EDWARD H. KAPLAN
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RALPH WILLIAM HUENEMANN. *The Dragon and the Iron Horse: The Economics of Railroads in China, 1876–1937*. (Harvard East Asian Monographs, number 109.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1984. Pp. xii, 347. \$20.00.

In this interestingly constructed study Ralph William Huenemann tries to assess the economic benefits of railroads in China from the beginning of modernization in the late Ch'ing to the outbreak of the Japanese war in the Nationalist era, a period marked by political change as well as social and intellectual ferment. Regarded by the Chinese both as instrument and symbol of their striving for national reconstruction, railroads assumed an increasing significance in public policy, international relations (through foreign railway loans), and private interests. It is pertinent to ask just what part the railroads played in the general context of national change. Critical of previous studies on Chinese railroads, Huenemann seeks to offer further insights into the subject by combining a historical review with econometric analysis of railroad performance in China.

In the opening chapter Huenemann presents a "fable" about the introduction of a rail line into a traditional economic region, thus defining the paradigm for his subsequent investigation. Chapter 2, "The Tempo of Construction, 1876–1937," is a well-written narrative that lucidly delineates the circumstances surrounding the financing, building, and operation of Chinese railways in that period; Huenemann skillfully interweaves the external and internal elements—such as the scramble for concessions, the rise of Chinese nationalism, and the impact of the warlord era—that impinged significantly

on the development of railroads. The next two chapters examine the role of foreign loans and other outside involvement in Chinese railroads. Huenemann argues to disprove the traditional charges against foreign railway loans as a negative factor in China's growth as a modern nation, pointing out that the financial costs were not excessive when considered in the context of international finance.

Nevertheless, in chapter 5 the author has to deal with the need to explain what contributed to the financial distress of the major railroads in China, for by 1930 nearly all national railways (that is, the main debtors for foreign loans) had been driven into insolvency, and only four lines were able to avoid default (p. 185). Huenemann attributes these difficulties to a variety of causes: stating that the "financial problems that plagued the Chinese railways were only partially due to imperialism" (p. 218), the author argues that domestic problems were more significant—including the effects of warlord activities, the problems of management that affected pricing policies and revenues, and unusual operating costs as circumstances demanded.

The final two chapters return to the overall assessment of economic benefits of the railways. It is true, as Huenemann points out, that financial performance is not the same as economic performance and that under some conditions "a railway may be losing money and still be contributing to the economy," whereas a profitable railway may not generate any economic benefits (pp. 219–20). Here the author stands tantalizingly on the brink of exploring the important question of industrialization and modernization vis-à-vis development, but he fails to do so. Had he taken that extra step, he would have provided his analysis with an additional theoretical dimension that would be helpful to those interested in studying the growth of modern nations. As it stands, his conclusions appear both unexceptional and hesitant. The railways did provide economic benefits to China, but, according to Huenemann, this is a conclusion of a "tenuous nature" (p. 247).

On the whole, however, this is a well-organized and carefully crafted book that is both informative and a welcome addition to the literature on the complex subject of early Chinese railways. The glossary regrettably contains a large number of errors that detract from its usefulness.

E-TU ZEN SUN

Pennsylvania State University

KUANG-SHENG LIAO. *Antiforeignism and Modernization in China, 1860–1980: Linkage between Domestic Politics and Foreign Policy*. Foreword by ALLEN S. WHITING. New York: St. Martin's or Chinese University Press, Hong Kong. 1984. Pp. xix, 333. \$27.95.

This is a bad book. Although Kuang-sheng Liao has put an enormous amount of work into it and has stated his thesis clearly, his conclusions are neither useful nor convincing. Such being the case, one may wonder why it should be reviewed at all. I do so because the subject is important, and Liao's approach promising enough, that others might take this book as a serious contribution to revisionist literature on modern China. We all seek that kind of contribution; unfortunately, this book does more harm than good to the cause of revisionism.

Briefly, Liao's thesis is that China's antiforeignism (which he defines as "hostile reaction to foreign countries, to be distinguished from xenophobia, 'fear of foreigners'" [pp. 8–9]) is a result of foreign threats and acts of aggression; it has little to do with China's political culture. He maintains, moreover, that antiforeignism is largely a creation of the government or the dominant party; that in a controlled society such as China it can be gauged by the frequency of mass demonstrations and the rhetoric of media items; and that it relates directly to internal politics, most notably, to factional struggles. Finally, antiforeignism is linked to modernization, at least, to political modernization. Although one may take issue with the above, a spirited argument would compel us to take this thesis seriously, even if we disagree with it at the end. But this book's premises, execution, and results are so questionable that one wonders how it came to be published at all.

In this short review one can only list the book's major shortcomings. Liao has the laudable aim of writing for both political scientists and historians, but he uses rather crude methods of treating his data and injects a heavy dose of presentism into his exposition. He counts mass demonstrations and items in the media (primarily for the period 1960–80) and presents them in tabular form in the text and in fifty pages of appendixes, but he gives no evidence that he used any of the generally accepted statistical methods of analysis. He recognizes that the earlier period (beginning with the 1860s) has no comparable data, and, accordingly, he uses a more traditional approach of narrative cum quotations. But, because he seeks to find early in the book (chaps. 2–5, which follow a chapter on conceptual framework and general theories) what he finds in the body of his work (chaps. 6–11, before a summary conclusion), his judgments, which are often correct, do not always come from his expositions. Liao also tries to link antiforeignism to modernization, but, because he defines the latter in exclusively political terms, his conclusions are virtually useless to those of us interested in modernization in its economic, social, and intellectual dimensions as well.

How could such a careful study go so far wrong? I believe Liao has failed to avoid the pitfalls that are common to us all. He cites basic works in social

psychology and political theory, but he apparently was not aware that even in the early 1970s when he wrote his dissertation some of his theoretical positions were being challenged and modified. Despite the lag of nearly a decade between writing and publication, the book shows little evidence of revision, other than some additional footnotes and citations.

Scholarly apparatus aside, I find it especially regrettable that Liao eschewed the comparative approach altogether. The question of the linkage of foreign and domestic politics is a universal one, and all nations manifest some antforeignism, which might have been usefully compared with that of China. Even though Liao demonstrates considerable historical awareness, the book abounds with statements like "Once the war with Japan ended in 1945, antforeign hostility ceased" (p. 236). One cannot close this review without mentioning that throughout the book Liao essentially equates antforeignism with anti-American acts or statements. Accordingly, he has relegated anti-Soviet actions to the chapter dealing with the Cultural Revolution. Perhaps the best one can say about this book is that it is almost a perfect demonstration of how not to turn a dissertation into a monograph. For that the publisher more than the author must be blamed.

SAMUEL C. CHU
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KEY RAY CHONG. *Americans and Chinese Reform and Revolution, 1898-1922: The Role of Private Citizens in Diplomacy*. Lanham, Md.: University Press of America. 1984. Pp. xiii, 308. Cloth \$24.75, paper \$13.75.

This book discusses the activities of the reformists led by Kang Yu-wei from 1898 to 1910 and of the revolutionaries led by Sun Yat-sen in the period 1910-22 and the efforts of both groups to obtain American aid through the instrument of private American citizens. Since not much scholarly work has been done on the roles of these Americans, the present work is a welcome addition. Unfortunately, its quality is rather thin and the result unimpressive. To begin with, the bibliography is too limited. Key Ray Chong made little use of the massive collections of materials published in China before and after 1949, and he does not seem to have spent much effort combing the available secondary sources in English. As a result, factual errors abound in the book. For instance, in its edict of abdication, the Manchu regime specifically granted Yuan Shih-kai plenipotentiary power to organize a temporary republican form of government—a fact that is in direct contrast to Chong's claim (p. 151). Again, Robert MacLane was the U.S. commissioner in

China in 1853-54 and therefore could not have played, as the author alleges, an important role in the management of the International Settlement, which was not formed until 1863 (p. 9). Again, according to the author, Kang Yu-wei was convinced in 1905 that the Manchu must be removed before legitimate Chinese reform could take place. If the term "Manchu" refers here to the dowager empress, Kang's conviction did not begin in 1905; if the word refers to the whole regime, then Kang, a lifelong loyalist, is turned into a revolutionary without adequate explanation.

Chong also seems to have an idealized, simplistic view of Sun's political life. Thus, he speaks of Yuan's usurpation of Sun's authority as the director general of railways, when, in fact, that title was given to Sun by Yuan as a political sop without any statutory power. Similarly, the author speaks of the American minister Paul Reinsch's consistent disregard of Sun's efforts to industrialize China. But Sun had little power in this period; practically all he did was intrigue, talk, and write—activities that can hardly be regarded as the industrialization of China.

The part of the book dealing with the actions of individual Americans is interesting. Nevertheless, it does not probe deeply enough. Here and there we see glimpses of some interesting facts; for example, James Deitrick's cultivation of both Sun and Sun's enemy, the Peking government, or Deitrick's refusal to surrender his power of attorney as Sun demanded. In neither case does the author attempt to analyze the inner details. Nor do the excerpts of correspondence in the appendix shed any light on these matters. Perhaps the data are simply insufficient. But then Chong had less reason to write this book.

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CHONG-SIK LEE. *Revolutionary Struggle in Manchuria: Chinese Communism and Soviet Interest, 1922-1945*. Berkeley and Los Angeles: University of California Press, for the Center for Chinese Studies, University of California, Berkeley. 1983. Pp. xv, 366. \$27.50.

Chong-Sik Lee is to be congratulated for his masterful investigation of the communist movement in Manchuria during the 1920s and 1930s. Of Korean extraction, Lee grew up in Manchuria and is competent in Chinese, Korean, Japanese, and Russian. He also has had the good fortune of having direct access to a number of previously unavailable documents of the Chinese Communist party (CCP).

With great care, Lee describes the various stages through which the Chinese communist movement in Manchuria progressed. He uses rare sources to

corroborate earlier findings in East Asian and communist studies. In particular, he employs the previously unavailable Ch'en Wei-jen Report, a CCP Central Committee document from 1927, to supplement a 1934 Japanese study (pp. 48–49). Ch'en Wei-jen was the first CCP cadre sent to Manchuria in 1923. He found the situation "dismal indeed," and he immediately attempted to reorganize the party apparatus. His task was greatly frustrated, however, by Soviet behavior there.

During the 1920s Soviet interest in many respects took precedence over the policies of the CCP with regard to Manchuria. While acting as a Soviet surrogate, the CCP made the protection of the Soviet Union its overriding priority. In Manchuria the results were catastrophic. The Chinese Communists were not allowed to antagonize the Japanese in any way, for the Soviet Union feared a Japanese reprisal. The CCP in Manchuria thus could not organize an effective party, for the masses there were violently anti-Japanese.

Surprisingly, after the 1929 Chinese Eastern Railway incident ended favorably for the Soviets, the CCP remained protective of them. Even though the CCP adopted both an anti-imperialist and an outwardly anti-Japanese stance on the Japanese takeover of Manchuria, it refrained from directly antagonizing the Japanese. The party feared that Manchuria was just a staging ground for an invasion of the Soviet Union. According to CCP propaganda, "an imperialist attack against the Soviet Union was imminent" (p. 135), and the Soviet Union, then the world's only champion for nonimperialist nations, had to be defended (p. 145).

The CCP leadership argued that "the struggle against Japanese imperialism was only an aspect—albeit a major one—of the struggles against all imperialists" (p. 131). Ironically, the CCP focused attention on the Kuomintang (KMT): "The KMT was the puppet of imperialism and the overthrow of the KMT was a precondition for anti-Japanese and anti-imperialist struggle" (p. 131). The CCP's defense of the Soviet Union was so fervent that it abandoned its more basic tasks of "organizing the anti-Japanese movement of the masses" (p. 175).

Only when the communist movement in Manchuria appealed to patriotism or to naked nationalism against Japanese imperialism—which was completely inconsistent with Marxist universalist ideology—was the CCP able to garner significant popular support. By the late 1930s, however, the Japanese military had "decimated" the nationalist guerrillas and, along with them, the Manchurian communist movement.

For one reason or another—perhaps because of strict orders from the Comintern, blind loyalty to the Soviets, or fear of the so-called counterrevolutionary KMT—the Central Committee's exclusively

pro-Soviet policies hampered a united front against the Japanese in Manchuria. Although the Comintern might have had some genuine concern for the spread of communism, the Soviet leadership ultimately did not much care what price the Chinese Communists had to pay to protect the USSR from Japan. Thus, Lee concludes, "If the Chinese Communist movement jeopardized Soviet security in the Far East, the former must be sacrificed for the latter" (p. 313).

The great depth and soundness of Lee's work makes an enormously important contribution to scholarship.

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Boston College

PENELOPE FRANCKS. *Technology and Agricultural Development in Pre-War Japan*. New Haven: Yale University Press. 1983. Pp. xiii, 322. \$30.00.

The title of this book seems to imply a dauntingly technical presentation, but such is not the case. Penelope Francks explains her material with such clarity of organization and expression that no reader of this journal would have any difficulty in following fully her account of technological change in prewar Japanese agriculture. The first third of Francks's book sets forth with great skill and insight the information needed as the context for her investigation: (1) the relation between technology and economic organization, (2) the special features of traditional Japanese agriculture, and (3) the changes that were taking place generally in Japanese agriculture in the late nineteenth century as well as the connection between those changes and what was happening in other sectors of the Japanese economy, particularly industry. The rest of the book is a detailed study of the technological change that occurred between 1850 and 1940 among the rice farmers of the Saga Plain (northwest Kyushu), a highly successful technological change that eventually gave those farmers the highest rice yields in Japan and transformed them "from subsistence producers, locked within the village economy, with their livelihood still largely dependent on the vagaries of weather, water and pests, into rather sophisticated commercial farmers, familiar with the latest machinery and the application of scientific commercial inputs to their fields" (p. 286). And all this happened without any important disruption of traditional village values or social structure.

Francks's message is, I believe, that this economically successful and socially smooth technological change was the outcome of people rationally promoting self-interest by making choices that responded to the opportunities and problems created by national economic changes but that were also

consonant with traditional village organization and agricultural techniques. A key to this achievement was, as she rightly emphasizes, the Japanese villagers' long experience in communal action, since this provided the vital avenue through which groups of small farmers could influence the character of technical development and share in its benefits. Equally important, however, is that villagers were free to make choices. Over the last hundred years there have been many examples where on the basis of doctrinaire economic or political theories governments have in the face of common sense forcibly and persistently imposed policies that disrupted the agricultural sector and led to stagnant or declining production. The Meiji government did not fall into that trap. After flirting with the large-scale cultivation techniques and machinery of Western agriculture, it opted to raise agricultural productivity by developing indigenous techniques, and it largely left to local initiative the precise nature and timing of the changes to be grafted on the traditional system. The government stood ready with financing and technological expertise, but implementation generally waited until a farm community from its own experience came to see and accept the need for change. The value of this already very valuable book would have been enhanced by a full explication of the reasoning and attitudes that led the Meiji leaders to adopt this policy.

ARTHUR E. TIEDEMANN
City College of New York

WILLIAM D. WRAY. *Mitsubishi and the N.Y.K., 1870-1914: Business Strategy in the Japanese Shipping Industry*. (Subseries on the History of Japanese Business and Industry.) Cambridge: Council on East Asian Studies; distributed by Harvard University Press, Cambridge. Pp. xx, 672. \$25.00.

This book is certainly to be numbered among the finest works on modern Japanese economic history. This is not merely a monograph on the first Japanese shipping company but also a persuasive narrative comprehensively and freshly describing how Japan grew from an economically backward country into an imperialist country. Few Japanese researchers can compete with William D. Wray's intensive research in archives located in Japan, the U.S., England, and Canada.

To explain the rapid economic modernization and expansion of Japan, Japanese scholars have emphasized the initiative of the Japanese government, whereas Western scholars have stressed the samurai spirit. By contrast, Wray centers on the entrepreneurial role of leading Japanese economists who produced proportioned business strategies. When Japan began modernizing in 1868, it was

ruled by severe, unequal treaties that deprived it of customs autonomy, and all of its long-distance overseas trade was carried by foreign shippers. According to Wray, the decisive factor enabling Japan to overcome these difficulties was an alliance among leading enterprises that had previously been hostile to each other. These enterprises took the initiative for obtaining subsidies from the government to develop national shipping lines bound for the Far East, America, and Europe; they also built up such key industries as shipbuilding and coal mining that later covered damages caused by fierce competition with Western shippers. Finally, these disparate firms effected a merger in 1885 that resulted in the establishment of the Japan Mail Steamship Company (NYK), which is still the predominant shipping company in Japan. By concentrating capital resources and talented personnel, the NYK ably countered foreign shippers and became the carrier for most of Japan's overseas trade. Thus, Wray has apparently applied Alfred Thayer Mahan's theory about command of the sea to examine the causes and process of Japanese economic modernization. Through brilliant documentation he has successfully established a remarkable concept of the independence of shipping, which, according to Wray, played the decisive role in Japanese economic expansion.

The second merit of Wray's book rests on his argument about how Japanese business (or economic) imperialism was shaped. From about 1900 on, the Yangtze valley functioned as a feeder for freight lines to the West; therefore, the NYK and other Japanese shipping firms aggressively penetrated there to win a larger share in carrying freight. The Japanese government supported them with subsidies because it hoped to enhance national security and to expand its sphere of influence. After all, by defeating Chinese and English firms in China, the Japanese won hegemony there and laid the foundation of Japanese economic imperialism. This viewpoint is important because it has long been neglected or underestimated in Japan. In comparison, Wray argues, China could not foster a national shipping company, since Chinese capitalists worked as compradors or continued to invest in foreign firms, and Western companies operating in the Far East could not expect efficient protection or support from their governments.

Wray does not refer to how Japanese business elites took the problem of nationwide mass poverty into account before forming their strategy. One hopes that Wray will examine this problem in the future, for Japanese scholars have been deeply concerned with this question and have uncovered much material.

RYUJI SASAKI
Tokyo Metropolitan University

R. B. SMITH. *An International History of the Vietnam War. Volume 1, Revolution versus Containment, 1955-61*. New York: St. Martin's. 1983. Pp. xiii, 301. \$25.00.

In this book R. B. Smith examines the Vietnam War from a world perspective, paying special attention to the interaction of developments in Vietnam with the crises of the cold war during the years 1955 to 1961. The idea of exploring relationships between external and internal factors is sound, but only insofar as the author does not lose sight of the centrality of the indigenous roots of one of history's most intense and violent conflicts. The book advances both the positive and negative sides of this delicate balance. On the one hand, Smith provides significant contributions to an understanding of the global context of decision making for Vietnam in the late 1950s, but, on the other hand, he is forced by reasons of space or chooses for other reasons to neglect the historical legacies of earlier years, which were so determinant for Vietnam.

The author, for example, gives inadequate information on and analysis of the effects of the refusal of Ngo Dinh Diem and his Washington backers to sign or follow the Geneva Agreements of July 1954. Nor does he fully discuss the effects of Diem's repression of all opposition, potential as well as actual, noncommunist as well as communist. Smith also makes insufficient reference to the bitter twenty-five-year struggle of the communists and others before 1954. That the surviving communist leaders averaged over seven years in prison before coming to power in August 1945 and then went on to defeat France in a bloody nine-year war made Vietnam a very special case. And that makes the author's conclusion too facile: "Trouble in Vietnam . . . was avoidable only in the same sense that the Korean War might have been avoided; by surrender. . . . There was a war in Vietnam because that was where the challenge arose, at a moment when Kennedy could not ignore the challenge" (p. 261). In fact, the war goes back at least to 1945.

Although the author warns that "we must be careful not simply to revert to the purely mechanistic view of 'world communism' that prevailed in the 1950s" (p. 10), he himself indulges in some doubtful "Kremlinology" of the sort that contributed to "mechanistic views," such as in his discussions of Hanoi's factions (pp. 99-101). He almost invariably accepts Washington's interpretations and, for the period after 1975, those of Beijing. There is also a curious neglect of French sources, notably, those of Philippe Devillers, Jean Lacouture, and François Joyaux.

These criticisms aside, the positive aspects make Smith's book welcome (as was the case for his 1968 work, *Vietnam and the West*). It is important to have in

one volume such chronologies of worldwide and Indochinese events and summaries of various crises (Berlin, Cuba, Indonesia, the Taiwan Straits, Thailand) and their relation to Indochina. Especially useful are his more extended treatments of the developments in Cambodia and Laos and their interaction with decisions made in the capitals concerned.

With regard to the Vietnamese communists, Smith also makes some good points, noting, for example, the policy shift symbolized by the founding of the National Liberation Front in December 1960, when Hanoi dropped its previous insistence on the unity of all parts of Vietnam. Even as Hanoi leaders continued to direct their then mostly "southern" comrades, they temporarily recognized a separate South for tactical reasons.

One hopes subsequent volumes can pay more adequate attention not only to contemporary global crises and their effects but also to decisive developments in Vietnam. I agree that both components are indispensable for understanding what happened in Indochina, but this work neglects the internal for the external and thereby commits a more serious error than its opposite.

JAMES P. HARRISON
Hunter College
City University of New York

SARVEPALLI GOPAL. *Jawaharlal Nehru: A Biography. Volume 3, 1956-1964*. Cambridge: Harvard University Press. 1984. Pp. 336. \$22.50.

In the vast library of writings on Jawaharlal Nehru, the magisterial biography by Sarvepalli Gopal, which is now complete with the publication of this third volume, deserves a special place. It is based on intimate contacts with Nehru and other leading personalities of the Nehru era, on an exhaustive examination of the Nehru papers (Indira Gandhi gave him unimpeded access to this unique collection) and other private and official papers, files, proceedings, and reports, on extensive interviews, and on a wide familiarity with secondary sources. Such a "life-and-times" biography is most appropriate for a man whose life was so interwoven with the country and the world in which he lived. This third volume covers the final eight years of Nehru's life (1956-64). These were eventful years of struggle and disappointment, but in the face of many adversities Nehru never lost his faith in the Indian people, human nature, or the path that he had charted for his country.

In dealing with the domestic scene, Gopal refers to a bewildering number of programs and developments and of interactions among people and organizations. Many of these relate to Nehru's concerns

with national and social integration and with economic development. There is a fascinating chapter on the circumstances leading to the dismissal of the Communist government in Kerala in 1959, a decision that, according to Gopal, "tarnished Nehru's reputation for ethical behaviour in politics and, from a long-term view, weakened his position" (p. 73). Another chapter, "Towards Cooperative Farming," is less satisfactory, mainly because it really deals with a variety of ineffective and half-hearted efforts "to change the face of Indian agriculture" (p. 118).

Gopal devotes even more space to foreign than to domestic affairs. But, aside from a discussion of India's deteriorating relations with China and two well-developed case studies of India's "Crusade in the Congo" and the Indian military takeover of Goa in December 1961, the treatment of India's external relations—even with Pakistan—is quite selective and sketchy.

Gopal presents effectively the Indian side of the story of growing tensions with China after 1959. He provides many revealing insights into Nehru's confused thinking about China and the incredible extent of India's unpreparedness—both military and psychological—for coping with the Chinese positions and actions. But he insists that during the entire period of crisis Nehru never lost his nerve. He does not even mention India's so-called forward policy in the months preceding the border war of late 1962. He is convinced that "the facts belie all later allegations . . . that it was India which precipitated a major conflict" (p. 213).

The concluding chapter, entitled "Full Stop," is a reflective essay on Nehru and his contribution to modern India. Gopal advances the rather contradictory conclusions that "the Nehru age . . . seems today a vanished world" (p. 301) and that Nehru's "influence is still a vital force in India" (p. 297). In Gopal's view democracy in India "is Nehru's most lasting monument" (p. 279). He believes that "Nehru was important as much for what he was as for what he strove to do" (p. 298) and that he was "one of the few great men of the age" (p. 300).

This splendid biography of "one of the few great men of the age" should be read and pondered by all who are concerned with the global *problematique* as well as by those who are interested in the history of modern India.

NORMAN D. PALMER
University of Pennsylvania

DERYCK SCARR. *Fiji: A Short History*. Laie, Hawaii: The Institute for Polynesian Studies; distributed by University of Hawaii Press, Honolulu. 1984. Pp. xv, 202. \$18.95.

Deryck Scarr is to be congratulated for having the courage to attempt something that few Pacific histo-

rians have dared to do: to write a general book for a general audience. Like many other islands in the Pacific, Fiji has been studied in considerable depth, and many specialist publications examine aspects of Fiji's past. But this book is the first general survey of Fiji's history from prehistoric times to the present day.

Nonspecialists are led gently along a predictable enough narrative path—from the nature of pre-European Fijian society, early cultural contact with European traders and missionaries, European settlement, and American and British involvement in Fijian affairs to annexation by Britain, the introduction of Indians, the economic and political issues of late nineteenth- and twentieth-century colonial rule, political independence in 1970, and, finally, the problems and prospects for a pluralistic society.

It would be too easy to point to issues or events that have been omitted in this short book, although I think it is fair to comment that readers may expect more than the six pages devoted to Fiji since 1970. And Indians, who for a long time have made up more than half of the total population of Fiji, certainly get less attention than their numbers may warrant.

Nonspecialist readers (especially high school students, who, in the foreword by Fiji's minister of education, are urged to read this book) may well find Scarr's rather elliptical and convoluted style of writing (which I myself quite like) rather difficult to follow. There are times when the richness and complexity of expression obscures some basic point. I imagine students seeking ready-made answers to basic questions—"Why did Fijians become Christianized?", "Why did Britain annex Fiji?", and "Why did Fiji gain independence in 1970?"—will not easily find them in this work. In this respect Scarr's book is more the story of Fiji than an analytical history of it.

These reservations aside, Scarr's efforts have been most worthwhile. We need more such books on Pacific islands to help introduce their history to a wider audience.

K. R. HOWE
Massey University

UNITED STATES

OLIVE PATRICIA DICKASON. *The Myth of the Savage and the Beginnings of French Colonialism in the Americas*. Edmonton: University of Alberta Press. 1984. Pp. xvii, 372. \$30.00.

Olive Patricia Dickason presents a searching overview of the many frontiers on which native Americans and Europeans first met. At one level, the study assesses the social dynamics that channeled the en-

counter. At another, it details the developing economic, political, and legal rationale for colonization. And Dickason frames her work with a fresh reexamination of the cross-cultural sources of what soon became the "Indian problem."

In the first section Dickason grounds the problem of Indian-white relations in the impressionistic anthropology of the sixteenth century. Christians viewed man as sinful by nature and urbane in culture. French culture, Dickason shows, was firmly rooted in an aristocratic and occupational stratification that duplicated the complex chain of being separating man from God. From the first contacts the French associated native Americans with a traditional, ambivalent symbol of nature: *l'homme sauvage*, the wild man of the woods.

Section 2 concentrates on the preconceptions and rationalizations that operated on both sides of the early frontier. Dickason reconstructs the relationship in fishing, trading, and diplomacy. Three aspects of these meetings especially stand out. She thoroughly examines the legal justifications for France's developing colonial policy in the light of European diplomatic rivalries, provides a provocative account of the critical Indian reactions while visiting Europe, and exposes the social values that informed both Indian and European responses.

In a final section Dickason examines the economic, political, and religious foundations of New France. Here, as throughout, her command of the ethnological record generates many new insights. Her view of the colony's development is particularly well balanced because she draws on an understanding of cross-cultural economics. For example, Dickason demonstrates that Indian redistributive economies were far more compatible with French commerce than is usually appreciated. Similarly, she not only relates French commercial pragmatism to the equally important missionary motivations but also shows that Indian sensibilities contributed much to the ideological agreement on which the religious alliance was grounded.

If much of this seems familiar, Dickason tells the story with revealing detail and comprehensive documentation. The book also reproduces many illustrations from contemporary sources that are, in themselves, invaluable for comprehending the encounter. All told, this is a major contribution.

KENNETH M. MORRISON
Arizona State University

KENNETH M. MORRISON. *The Embattled Northeast: The Elusive Ideal of Alliance in Abenaki-Euroamerican Relations*. Berkeley and Los Angeles: University of California Press. 1984. Pp. x, 256. \$24.95.

In this book Kenneth M. Morrison seeks to write the history of relations between the English, French,

and Abenaki peoples in Maine during the seventeenth and early eighteenth centuries. His central ethnohistorical premise is familiar: in their alliances with the French and English, the Abenaki, like other native peoples confronted by invading Europeans, "invariably sought their own domestic and diplomatic goals," especially those that embodied their notions of social good (p. 2). Far from being passive victims dominated by more powerful opponents, the Abenaki shrewdly adjusted their religion, their political community, and their treaty relations to preserve their autonomy.

Morrison is committed to a fundamentally idealist interpretation of Abenaki-European relations. For him, the English, French, and Abenaki each embraced radically different value systems, and these more than anything else determined the ways they responded to one another. His account thus pays less attention than a number of recent studies to such factors as disease and trade, emphasizing instead religious interaction and the noncommercial aspects of diplomacy. At its best, the book demonstrates how useful the idealist perspective can be. For example, by analyzing Abenaki conversion to Christianity in the context of native belief systems, Morrison is able to shed new light on the region's Jesuit missionaries, arguing that they achieved their success by becoming the equivalent of Christian shamans. The priests were thus instrumental in helping the Abenaki adjust their beliefs to a new syncretic religious system, and some, like the Jesuit Sebastien Racle, even went so far as to help reorganize Abenaki political alignments to encourage military resistance.

The book is solidly researched and makes wide use of the available literature. Unfortunately, the limitations of the documents frequently prevent Morrison from fully achieving his goal of discussing "comparative social values" (p. 6). Given the "subjective character" of his subject, one regularly wishes for more supporting evidence, especially direct quotations from the Indians themselves, to support Morrison's claims. For instance, he portrays the Abenaki living in almost wholly benign polities in which the highest goal for any leader was the good of the community. Abenaki sachems, he argues, were not interested in power, capital accumulation, or self-aggrandizement but instead sought only "security achieved for the community as a whole" (p. 29). Through a system of even-handed redistribution, he claims, the sachems "not only created a surplus but assured its fair division" (p. 29) (one wishes Morrison had spent a little more time exploring the meaning of that phrase, "fair division"). The resulting image of Abenaki political life is remarkably free from conflict and seems directly to support Morrison's claims on behalf of the Abenaki's commitment to altruism, noncoercion, and community

solidarity. Whether or not one accepts that portrait—and those with more materialist biases are likely to be at least a little suspicious of it—one has to admit that there is little direct evidence either for or against it. In this, Morrison is perhaps not much different from others who have tried to use the fragmentary documents of this period to explore Indian history, and his book makes a solid contribution to the field.

WILLIAM CRONON
Yale University

GARY CLAYTON ANDERSON. *Kinsmen of Another Kind: Dakota-White Relations in the Upper Mississippi Valley, 1650–1862*. Lincoln: University of Nebraska Press. 1984. Pp. xvi, 383.

Minnesota's Sioux uprising of 1862, one of the bloodiest conflicts in the long history of Indian-white contact, came after two centuries of largely harmonious relations between the Dakota Indians of the Upper Mississippi Valley and the Europeans and their American successors. Ever since then historians have tried to discover what went wrong. Most serious investigators have suggested a multiplicity of causes and have disagreed only on the relative weight to be given to the various factors responsible for the apparently sudden turnaround in the Indians' attitude toward the whites.

In this book Gary Clayton Anderson advances a theory that earlier scholars have not so much overlooked as underemphasized; in redressing the balance, he stresses the role of kinship as the crucial element in Indian-white relationships. According to his thesis, until well into the nineteenth century, non-Indians who entered the Sioux homeland were not perceived as intruders or potential enemies. Rather, they were seen as bringers of useful additions to the native cultural inventory and in many cases were adopted into the tribe as "fictive" kin.

As long as whites could be seen as entering the Dakota world on native terms, relations between the races were friendly and, on the whole, beneficial to both. But, when large numbers of Euro-Americans entered Minnesota unwilling to form kinship ties with the native inhabitants or to abide by the reciprocal obligations such ties involved, these settlers came to be perceived as a menace to the integrity of Dakota society. Coupled with the environmental damage incident to the fur trade—the near-extirpation of the game animals on which the Dakota people had subsisted—and the factionalism engendered by the assimilationist pressures from missionaries and government officials, these changes created a climate in which a comparatively minor incident could set off open warfare.

Anderson may give disproportionate weight to the kinship theory as the single most important determinant of Indian-white relations over the centuries, but it is difficult to fault a book based on an examination of every conceivable documentary source. The book is obviously the labor of many years, and the author has bestowed more time on his subject than most scholars who are also teachers can afford to spend on such research. Hence, one is inclined to trust his judgment and to accept his conclusions as authoritative.

Flaws are few and minor. On page 191, for example, "agent McLeod" is mentioned—an apparent confusion of agent Nathaniel McLean with trader Martin McLeod. And the history of the eastern Sioux after the uprising of 1862 is summarized on page 278 with some departures from fact. But these apparent errors do not in any way detract from the overall validity of Anderson's interpretation. Any future synthesis of Dakota history will have to make use of his findings, either by accepting them in toto or by at least giving them a more prominent place than they have hitherto enjoyed.

ROY W. MEYER
Mankato State University

JOHN FREDERICK WOOLVERTON. *Colonial Anglicanism in North America*. Detroit, Mich.: Wayne State University Press. 1984. Pp. 331. \$30.00.

As the editor of the *Historical Magazine of the Protestant Episcopal Church*, John Frederick Woolverton has acted as a midwife for the new revisionist scholarship dealing with the Church of England in the American colonies. In the book under review he synthesizes that material in an effective manner and adds to it in some important ways.

Woolverton's main theme is the Anglican drive for religious uniformity and its frustration by the inherently heterogeneous nature of colonial religion. He begins with seventeenth-century Virginia, emphasizing the Puritanism that was part of Anglican roots there and the laicization that occurred as parish vestries won control over their clergymen. The story then moves to the Anglican offensive at the turn of the century when the Society for the Propagation of the Gospel was founded and began sending the first of the 329 clergymen and teachers that it eventually supported in the American colonies. Woolverton describes the development of Anglican establishments in the southern colonies in the eighteenth century and the emergence of a vigorous Anglican minority in New England and the Middle Colonies. His final chapters, dealing with the Great Awakening, the Enlightenment, and the American Revolution, tell the complicated tale of how Anglicans failed to dominate the religious life of the

colonies and instead adapted to become a successful denomination.

Woolverton's interest lies in the "interaction of theology, politics, and the personality of individuals" (p. 9). His theological expertise is especially welcome, in part because many students of Anglicanism have been colonial historians more interested in social history than in religious ideas. In his fine chapter on Samuel Johnson and in many other parts of the book, Woolverton provides convincing evidence of the intellectual contribution that colonial churchmen made to American culture. He also has a gift for capsule biography that makes individuals come alive as he describes their ideas and actions. This is as true for well-known personages such as Johnson, James Blair, and William Smith as it is for lesser figures such as Gideon Johnston, William Vesey, and Thomas Bradbury Chandler.

Despite the virtues of the book, something is missing: Woolverton tells us almost nothing about the colonial laity. He discusses the vestry as an agency of lay power in the South, but he ignores its governmental role and its social significance; the northern vestry is ignored altogether. Church life is given a few lines in the introduction, but, in general, parishioners are not a part of this account. Certainly, no author can do everything, and Woolverton is very clear about his own choices. It is difficult, though, to assess the effectiveness of a religious organization without reference to the people whose allegiance and involvement are the most important criteria for success.

Despite the absence of social perspective, this is an excellent study. It is the best general study of colonial Anglicanism that we have or are likely to have for some time. Scholars and laymen alike will find it a rich, readable, and reliable account.

S. CHARLES BOLTON
University of Arkansas

BRUCE C. DANIELS. *Dissent and Conformity on Narragansett Bay: The Colonial Rhode Island Town*. Middletown, Conn.: Wesleyan University Press. 1983. Pp. xii, 137. \$25.00.

Early Rhode Island has attracted neither the attention nor the enthusiasm historians have lavished on the rest of New England. In his celebrated essay "Errand into the Wilderness" (1956), Perry Miller made reference to "that cesspool then becoming known as Rhode Island," and most subsequent historians have, with varying degrees of disdain or indifference, more or less followed suit. Now, in the first modern study of the colonial Rhode Island town, Bruce C. Daniels attempts to move from stereotype to substance, to analyze the evolution of local government in the colony, and to place his

findings firmly within a comparative New England framework. The result is a useful if somewhat lackluster book.

Daniels's main thesis is that Rhode Islanders "made the transition from Puritan to Yankee in the act of founding their colony" (p. 113). The acquisitiveness, toleration, democracy, and urbanization normally associated with the nineteenth century were all present to some degree in early Rhode Island. So too were tax revolts, welfare crises, and a general flight from communal responsibility. Describing the nether side of Rhode Island's individualism in quasi-Darwinian terms, Daniels asserts that inhabitants "who could not compete either perished or were isolated in institutions" (p. 90). The early presence of strong and often difficult personalities, such as Samuel Gorton, Ann Hutchinson, and William Coddington, and the absence of a shared religious vision resulted in a "fragmented, quarrelsome, obstreperous society that alternated between defying and ignoring authority" (p. 21). Mutually devoted to liberty of conscience and to natural rights constitutionalism but to little else, townsmen fought the colony government over taxes, the courts over jurisdiction, and each other over land allocation. Although twice as likely as the inhabitants of Connecticut to vote in colonywide elections, Rhode Islanders shirked the responsibilities of office holding, attendance at town meetings, and care for the destitute. Paid civil servants, not town fathers, controlled local governance. Town councils held well-publicized meetings, kept careful minutes, and took formal votes—legalistic and bureaucratic tendencies Daniels finds to be characteristic of the colony's general political culture.

In possession of an extensive coastline but limited land resources, Rhode Island quickly became the "ocean colony," oriented toward long-distance trade and commercial expansion. Daniels finds that the three principal characteristics of eighteenth-century Rhode Island—a high rate of annual growth, population density, and ethnic diversity—were all attributable to the colony's reliance on trade. A doubling of the population from 1730 to 1748 combined with war-related disruptions of the Atlantic economy to swell the ranks of the poor, resulting in a sharp rise in warnings-out and the establishment of a system of workhouses. Financial success and personal independence may have come easier in Rhode Island than elsewhere in New England, but the burden of failure and dependency was nowhere heavier.

Daniels may exaggerate the degree to which Rhode Island was "born modern," riven by unfettered individualism and self-seeking—the colony, after all, was founded by radical Puritan sectarians—but this book, along with the work of Sydney James, should help bring Rhode Island at last into

the mainstream of colonial New England scholarship.

STEPHEN INNES
University of Virginia

PAUL MERRILL SPURLIN. *The French Enlightenment in America: Essays on the Times of the Founding Fathers*. Athens: University of Georgia Press. 1984. Pp. xi, 203. \$20.00.

Some problems never go away. Fashions and interpretive models may change in intellectual history, but, whether one studies individuals psychoanalytically or one prefers to examine the wider culture in terms of "text" and "figuration," one sooner or later has to tackle the question: Who was exposed to what idea, when, and what did he do about it? And nowhere in American intellectual history has this question been more persistent than in the study of the American Enlightenment. We have come a long way from the days when we either assumed that "Enlightenment ideas" were "in the air" or wondered out loud whether Europe exerted any significant influence on American thought at all. Students of intellectual history have tried to be less intuitive and more precise in discussing the influence of ideas. Whether the precision is worth the price remains to be seen.

Paul Merrill Spurlin has written a great deal about the influence of French thinkers (most notably, Rousseau and Montesquieu) on eighteenth-century Americans, and he has now brought his thoughts together in this book. Spurlin examines such themes as American knowledge of French, the Founding Fathers' attitudes toward France, the sale and distribution of French books in America, and the specific influence of men like Buffon, Voltaire, Diderot, and Condorcet. Spurlin not only draws on his own research but also makes use of the latest efforts to measure, quantitatively, such things as literacy and popular exposure to new ideas.

Spurlin's conclusions seem eminently plausible. America's French connection waxed hot and cold during the eighteenth century, depending on the diplomatic relations between the two countries. American admiration for things French reached a highpoint toward the end of the century, cooling considerably once the French Revolution turned bloody and radical. Some French intellectuals, like Montesquieu, enjoyed an enthusiastic American following, mainly because certain of their theories (like the tripartite division of government) confirmed American practice. Condillac, however, was admired for his high-mindedness but overlooked as a theorist: Americans did not need a theory of progress. Rousseau's *Emile* enjoyed "a vogue but not success," whereas his *Social Contract* was ignored.

Spurlin's book charts the boundary of permissible generality in treating the influence of French thought in eighteenth-century America. It performs an important service and will have to be consulted by anyone who writes about the Enlightenment in America. It offers nothing especially startling, but then, as Voltaire once said, human knowledge does not usually advance by thunderclaps.

DONALD H. MEYER
University of Delaware

JOHN R. ALDEN. *George Washington: A Biography*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1984. Pp. xii, 326. \$19.95.

As one expects from a distinguished senior historian of the American revolutionary era, John R. Alden's biography of George Washington is well informed, workmanlike, and judicious. The book covers Washington's life from his complicated upbringing amid assorted relatives and gentry along the Rapahannock and Potomac rivers to his death (of "strep throat") at Mount Vernon in December 1799. Along the way are succinct and reliable accounts of his strenuous military career during the French and Indian wars, his maturing as a prosperous planter after his marriage to Martha Custis in 1759, and then his heroic and transcendently important careers as commander in chief during the American Revolution and as first president of the United States. Alden takes advantage, of course, of the majestic Washington biographies of the last half-century by Douglas S. Freeman and James Thomas Flexner, as well as the many recent, fine biographies of Washington's famous contemporaries. In general, Alden's view of Washington is sympathetic and leaves him the towering, almost-too-good-to-be-true father of his country familiar from the pages of Freeman and Flexner. The places in Washington's life where iconoclasts have found flaws—the alleged affair with Sally Fairfax, his lusting for a commission in the British army, tactical blunders during revolutionary war battles, and alleged Federalist partisanship during his presidency, for example—are all examined, and to some degree acknowledged, but put in a perspective that leaves them much overshadowed by his marital and familial fidelity, his decency in pursuit of ambition, his growth in military genius, and his key role as the leader above party in the new nation.

Stylistic infelicity and a certain lumbering uninterestingness, however, make the book a solid contribution rather than an exciting masterwork on Washington. When all is said and done, little in the book is new or even very interesting—the familiar revolutionary scene Alden has written about before

is there, and Washington strides through it as he must in any account of the era. In discussing Sir William Howe's plight in occupied Philadelphia in 1777-78, Alden notes that the general was "not excessively addicted to cerebration" (p. 141)—why such a ponderous phrase? Again and again Alden's sentences fatigue the reader and becloud his mind. And it is surely gratuitous to remark, after noting that Martha Washington usually joined her husband during winter encampments, that "many soldiers had less formal reunions with camp followers" (p. 121). Is that meant to demean Martha or her husband, the soldiers or the "camp followers"? Fortunately, such lapses do not detract from this careful recounting of the life of the only indispensable figure among the nation's founders. Students and general readers will benefit from a thoughtful biography by a thoroughly qualified author.

RALPH KETCHAM
Syracuse University

CHARLES G. STEFFEN. *The Mechanics of Baltimore: Workers and Politics in the Age of Revolution, 1763-1812*. (Working Class in American History.) Urbana: University of Illinois Press. 1984. Pp. xv, 296. \$24.95.

Charles G. Steffen's study of the artisans of Baltimore in the revolutionary era contributes to the growing number of studies of the conditions, consciousness, and culture of America's preindustrial craftsmen. Steffen's findings parallel those of other scholars who have worked this terrain. He shows that the political activity of artisans grew rapidly in the revolutionary era and that they upheld their own notions of what was meant—or ought to be meant—by republicanism. Their cohesiveness as a political group began to crumble under the impact of protoindustrialization in the late eighteenth and early nineteenth centuries, and the middling and lower artisans became a mainstay of the emerging Jeffersonian party after their disillusionment with Federalist economic policies in the early 1790s. Finally, as the community of interest between masters and journeymen broke down in the early nineteenth century, the latter organized and protested militantly. Along with other studies, this book drives home the points that the political involvement of the urban mechanics was a powerful force moving the colonies to rebellion and that city artisans were in the forefront of calls for democratic reforms in American society.

Where Baltimore differed from other urban centers was not in the ideology of its artisans but in their relative weakness in the arena of provincial politics. Steffen's evidence does not always support his two major contentions—that Baltimore's tradesmen de-

veloped into "a cohesive political force" between 1763 and 1800 and that the political mobilization of artisans was "a crucial factor shaping the major events of the period" (p. 171). It appears, in fact, that at no time in the last third of the eighteenth century were artisans wholly unified; all too often, as Steffen details, splits in their ranks contributed to the defeats the mechanics suffered in trying to democratize city government, erect a mechanics bank, and elect men of their own kind to office.

Steffen sensibly seeks to balance the material and ideological factors impinging on artisans' lives. Two chapters of this book are especially noteworthy in this regard. Using unusually complete records of indenture and apprenticeship, he traces the postrevolutionary degradation of labor in shoemaking and tailoring, as children and women working for pitiable wages flooded into shops to fabricate "slop goods" for a mass market. In the final chapter the author traces the rise of Methodism in Baltimore and analyzes its uneven appeal among working-class families. His treatment, which invites comparison with E. P. Thompson's acerbic indictment of Methodism among the English working class, shows the complexity of labor's Methodist connection. On the one hand, Baltimore's more successful mechanics were much attracted to Methodism's "democratized doctrine of salvation" and responded to its invitation to working people to participate in leadership roles. On the other hand, by stressing discipline, thrift, and industry, Methodism tended to set the upper artisans against those below them, for the latter's subculture, as seen from above, revolved far too much around the tavern, gambling den, brothel, and cockfight.

GARY B. NASH
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Los Angeles

RICHARD R. BEEMAN. *The Evolution of the Southern Backcountry: A Case Study of Lunenburg County, Virginia, 1746-1832*. Philadelphia: University of Pennsylvania Press. 1984. Pp. xvi, 272.

Only in the last decade have colonial social historians turned their attention southward, away from the tidy and law-loving towns of New England. Like the St. Marys (Maryland) Commission scholars and Darrett Rutman, Richard R. Beeman is convinced that local history can penetrate mysteries that broader studies pass over. Beeman seeks to expand scholarly boundaries, to pioneer, as it were, in frontier Virginia history. This is certainly a worthwhile objective.

Beeman points out that in mid-eighteenth-century Southside, first settlers, mostly poor and transient, could not replicate eastern culture and

institutions. The country's communal development was glacial and hindered by both labor shortages and the economic disadvantage of unnavigable waterways. Central to his story is the allegedly class-based dispute between oft-times absentee Anglican landlords and dissenting upstarts, a rivalry and interpretation familiar to Rhys Isaac's readers.

Beeman does not realize, however, the distinctiveness of the frontier as fully as his documents must have suggested. He describes Southsiders as proto-Yuppies: conservative opportunity seekers, indifferent about race and willing to exploit all advantages. I exaggerate. Nonetheless, we sometimes do remake colonials in our own image. To his credit, Beeman stresses the movement toward commercial cropping, but, unlike the old Turnerians, he neglects the hunting culture that briefly supplemented subsistence farming and livestock tending. Minor though it may seem to us today, game was important to Southsiders for commercial, alimentary, and recreational reasons, especially before tobacco-cropping encroachments on wastelands. In genuine distinction to Tidewater refinements, these backland folk, like their seventeenth-century forebears, knew more about hound dogs than wines, more about deer, wolf, and opossum than about theology or politics. Social rank had as much to do with who could hunt, race, drink, gamble—and marry—with whom as it did with male church affiliations, especially in the county's prerevolutionary days.

As Beeman stresses throughout, Southsiders never replicated the grandeur of William Byrd's world at Westover. But we have too long romanticized Tidewater life. Beeman recognizes the danger (p. 238 n. 3) but proceeds with comparisons anyhow. Moreover, he too slavishly forces Lunenburg history to fit Isaac's hypothesis on sectarian warfare. Underplayed are such issues as personal violence, kinship, friendship, marital patterns, and patronage. Although highly useful on housing and wealthholding questions, the work provides only glimpses of white Virginia life and offers little on black culture. Are court records totally silent on these and other matters? If so, then Lunenburg was an unhappy choice.

Beeman is best at describing economic change and the progress of slaveholding. His description of how nineteenth-century Southsiders pridefully became "southron" enthusiasts for slavery and states' rights cannot be gainsaid. Although an exemplary craftsman, Beeman could have forsaken well-trodden paths for more venturesome explorations of the wildwood even at risk of snakebite and tick fever.

BERTRAM WYATT-BROWN
University of Florida

HAL S. BARRON. *Those Who Stayed Behind: Rural Society in Nineteenth-Century New England*. (Interdisciplinary

Perspectives on Modern History.) New York: Cambridge University Press. 1984. Pp. xiii, 184. \$24.95.

To complement scholarly studies of frontier communities and industrial cities of the nineteenth century, Hal S. Barron investigates what might be called the dynamics of stability. In an attempt to combine quantitative analysis with a narrative style, Barron focuses on Chelsea, Vermont, an agricultural township of fewer than two thousand people. To achieve his aim, he mines a rich lode of country, state, and federal documentary sources and provides a sophisticated array of tables on mobility, wealth, and persistence of residence. Unfortunately, however, he discovered only two diaries kept for brief periods, few private letters, and no village newspaper.

In his opening chapters Barron critically assesses past and current literature on the purported devolution of rural northern New England. He finds particular fault with the "Country Life" movement, a coterie of educated, middle-class critics, such as Wilbert L. Anderson and Gifford Pinchot, who zealously deplored rural decline and urged "scientific" reforms.

A close examination of the realities of depopulation constitutes the core of this study. What Barron did not find in Chelsea was rot, decimation, or an abandoned countryside. Indeed, constant out-migration, decreasing in-migration, and intermarriage with neighbors or nearby farmers produced community stability. Despite population loss after 1850, the twenty-four thousand acres of Chelsea township remained divided into approximately two hundred farms, each about one hundred twenty-one acres in size and cultivated by the farmer-proprietor and his family. Only two major economic shifts occurred in the nineteenth century: the "Merino mania," beginning in the 1820s and the introduction of commercial sheep raising, and the growth of dairying in the 1870s. Yet neither innovation eliminated the production of other crops or livestock, and, despite a drastic fall in wool prices and an increasing market for butter, Chelsea husbandmen did not turn exclusively to cattle raising. The reason was simple. Beset by chronic labor shortages, farmers did not have enough farmhands to milk large herds of cows twice daily. The competency ethic continued to prevail over the profit ethic into the twentieth century.

This traditional mentalité, moreover, served a way of life and preserved both farm and family. Since land was the principal form of wealth, the typical Chelsea father bestowed on each of his four elder children gifts and dowries sufficient to serve as down payments on farms. At about age 65, he sold his own estate on generous terms to his youngest son, who was then in his midtwenties and ready to

settle down. In return, the "Benjamin" agreed to maintain his elderly parents and unmarried sisters and to assume the farm's liabilities.

The system of property transfer led to a homogeneous and progressively older population of family farmers, tied to the land and to each other. Barron claims that a sharp reduction in divisiveness followed. As the century closed, Protestant denominationalism and discipline, support for temperance, adherence to Republicanism, and a lively level of associational activities built consensus. But Barron only examines the impact of larger movements on the Chelsea polity and does not investigate village conflicts over such issues as schools, welfare, and roads nor the sometimes contentious roles of dominant families or individuals within his "peaceable kingdom."

Nevertheless, Barron skillfully fills a void in our understanding of the American community. The village that "stayed behind" was neither deserted nor decayed. In contrast to the raucous western town or the tumultuous eastern city of the nineteenth century, Chelsea marched slowly to a different drumbeat—staid, sober, stable, and serene.

ESTELLE F. FEINSTEIN
University of Connecticut,
Stamford

ROBERT A. FERGUSON. *Law and Letters in American Culture*. Cambridge: Harvard University Press. 1984. Pp. viii, 417. \$22.50.

We seem to be entering a renaissance of American studies. Robert A. Ferguson has produced a splendid, wide-ranging study of crucial aspects of American culture in the period from the revolution to the Civil War. Taking off from Perry Miller's brilliant discussion of legal thinking in *The Life of the Mind in America* (1965), Ferguson posits a vital "configuration" of law and letters that allowed lawyer-writers to dominate the thought and literature of the early republic. This configuration is used as a hermeneutic device to open up a new angle of vision on familiar figures who all spent some period of their lives as lawyers: Thomas Jefferson, Charles Brockden Brown, Washington Irving, William Cullen Bryant, Daniel Webster, and the Richard Henry Danas (father and son), along with others treated in less detail. Ferguson demonstrates the ways in which a legal style of argument impressed its own esthetic on literary expression (there is a notable explication of *Notes on the State of Virginia* in these terms) and how law and literature were united through the intellectual influence of classicism.

After the revolution lawyers, not clergymen, were the natural guardians and interpreters of the republic. Using Cicero as the ideal, the lawyer was sup-

posed to be a man of wide learning as well as practical acumen, and his highest calling was to speak beyond the court to the people and be eloquent on themes of vital importance to the nation. The connection between law and letters was as natural as that between law and politics. The two careers could be combined because they shared the same task: to define the new nation to itself in terms of rational liberty. Even those who, like Brown, Irving, and Bryant, found the practice of the law distasteful and broke away could not escape the fear that to follow the lure of "mere" literature was to betray the Ciceronian ideal of the active life of civic responsibility.

The unified culture of the early republic, in which no radical disjunction existed between the claims of professional life and the claims of citizenship or between art and politics, began to crumble by the 1830s. As the law became increasingly technical, lawyers became technicians in a hermetic professional world. New voices out of Concord, not nurtured in or harnessed to the law, challenged the neoclassical style and the Ciceronian values with the Romantic claims of the individual self. Finally, the hegemony of the legal intellect was fatally undermined by its inability to cope with the great moral question of the period: slavery. Ferguson ends with a brief discussion of Lincoln. A lawyer in the older classical, generalist mode, Lincoln could enunciate the fundamental moral principles of the republic because, though committed to the law, he knew that law rested ultimately on something other than itself.

This gracefully written book reveals wide reading and a subtle sensitivity. It is essential reading for anyone interested in the intellectual history of the early republic.

JEAN V. MATTHEWS
University of Western Ontario

RICHARD K. MATTHEWS. *The Radical Politics of Thomas Jefferson: A Revisionist View*. Lawrence: University Press of Kansas. 1984. Pp. ix, 171. \$20.00.

If any scholarly society awards a prize for cheek, I want to nominate this book. Richard K. Matthews begins with a shrewd review of the varieties of Jeffersonian scholarship, charging that most scholars read Jefferson selectively and find in the vast and varied corpus of his writings evidence that confirms their preconceptions. On as limited an evidential base as any I can readily recall, Matthews then develops his interpretation, a reading consciously intended to enlist the third president in a radical critique of "the market liberal democracy of the American political system" (p. 120). After warning that the task of understanding Jefferson, who never wrote a systematic treatise, is greatly complicated by

the bulk and fragmentary character of his surviving statements, Matthews nevertheless insists on treating this constructive politician as a theoretical philosopher.

I do not mean to write a uniformly negative review—far from it. This is a bold, provocative, and often quite insightful work. The product of a penetrating mind and a forceful pen, it challenges both liberal and classical republican interpretations of the democratic statesman. It offers us a Jefferson whose “humanism,” “communitarian anarchism,” and “radical democracy” do “make his views stand as an alternative to the market liberalism of the past and present” (p. 122). It can be read with profit by everyone who has a good acquaintance with its subject.

General readers, however, deserve some words of caution. Although it is possible to extract this sort of Jefferson from some of his more speculative writings, the thinker who emerges is only distantly related to the historical architect of American traditions. Nearly every specialist will argue with Matthews’s readings of the documents, and historians will insist that a reliance on these documents alone can only result in a radical simplification of Jefferson’s views. This problem is compounded, I would add, by what appears to me a limited familiarity with the writings of contemporaries to whom Jefferson is compared, a limitation most apparent in the footnotes to chapter 6. Matthews’s revisionist interpretation is a valuable and challenging addition to the body of existing literature, but it should not be swallowed in its undiluted form.

LANCE BANNING
University of Kentucky

JOHN C. GREENE. *American Science in the Age of Jefferson*. Ames: Iowa State University Press. 1984. Pp. xiv, 484. \$24.95.

The history of early American science has been well served. To Brooke Hindle’s *The Pursuit of Science in Revolutionary America* (1956) has been added Raymond P. Stearns’s *Science in the British Colonies of North America* (1970) and now this long-awaited volume by John C. Greene, author of *The Death of Adam* (1959). It is a difficult topic, because American science in what Greene calls the age of Jefferson (approximately the period from 1790 to 1830) had grown far more complex and certainly far more diffuse than it had been in the colonial and revolutionary periods. Stearns focused on the Royal Society of London and the American correspondents of its fellows; Hindle, on the Anglo-American natural history circle, the American Philosophical Society and its Philadelphia predecessors, and activities in a handful of eighteenth-century colleges. Greene

must deal with these and other elements and add to them a much larger institutional setting and geographic scope. His is an account of American science in that he includes Charleston, Cincinnati, Lexington, and Louisville, as well as lesser centers in the South and West, and does not confine himself to Philadelphia, New York, Boston, and New Haven.

Greene has written an account that is neither solely internalist nor solely externalist, and he has imposed no overarching thesis. The book begins with a survey of the American and, to a lesser degree, European social and institutional context. Scientific inquiry in America continued in a state of colonial dependency until about 1820, by which time, according to the author, it entered into partnership, however junior, in the broader efforts of an Atlantic scientific community. That dependency, both for patronage and for scientific ideas, continued to be on England, even though Continental science was increasingly accepted without an English filter. Certainly, French chemistry dominated easily, even with the presence in the United States of Joseph Priestley.

Greene surveys each discipline separately: astronomy, chemistry, geography, geology, botany, zoology and paleontology, physical anthropology, archaeology, and comparative linguistics. He deliberately ignores medicine on the ground that there was no medical science, although he does not ignore physicians, who constituted the majority of trained scientific practitioners, editors, and teachers throughout the period. His work is based on a judicious use of the secondary literature, on institutional and personal manuscript collections, and on a reading of the frequently ephemeral journals of short-lived societies as well as on the few that had a longer existence. This reviewer particularly admires Greene’s use of the rich holdings of the American Philosophical Society for his treatment of the study of Indian linguistics. For an age in which there were few true specialists, the science-by-science approach does create the problem of almost unavoidable repetition, and the same names reappear under many headings. This is a minor matter, though, in a work that is likely to remain the standard treatment for a long time.

GEORGE F. FRICK
University of Delaware

JAMES P. RONDA. *Lewis and Clark among the Indians*. Lincoln: University of Nebraska Press. 1984. Pp. xv, 310. \$24.95.

The Lewis and Clark Expedition caught not only the imagination of nineteenth-century Americans but the scholarly interest of twentieth-century historians as well. For the past fifty years books and

articles have appeared, each discussing some new aspect of the 1804–06 enterprise. Because of that work the temptation is to wonder if the explorers' activities deserve further analysis. According to James P. Ronda's study of how Lewis and Clark dealt with the Indians, there is still plenty to be learned. Among the many items written about the explorers, this book is the first that makes a serious effort to examine the motivations of the native Americans along the expedition route. The narrative considers the expeditions' objectives, methods of dealing with Indian leaders, understanding of the aboriginal situation, and degree of success or failure. In so doing, the author focuses on the Indian side of the story to an extent never before tried and with a high degree of success. The result is a sophisticated study of Indian-white relations in the early nineteenth-century West.

Ronda discusses the thorough efforts made by Thomas Jefferson and the explorers themselves to insure their psychological, intellectual, and physical preparation for the expedition west. The primary goals of Lewis and Clark included making peace with each tribe along the route, persuading the Indians to shift their allegiance and trading from the British to the Americans, urging peaceful relations with their tribal neighbors, and recruiting delegations to visit Washington. Jefferson also wanted Lewis and Clark to gather ethnological data of all sorts. Although this task proved difficult at times, it provided the explorers with their only notable success in Indian relations. The author demonstrates the contributions of Indian guides, hosts, informants, and suppliers to this process.

Several themes emerge as the central focus of this study. Despite concerted efforts to understand and accommodate Indian practices, the explorers brought with them cultural biases that prevented them from recognizing the significant motivations of their tribal hosts. Efforts to simplify existing tribal patterns of diplomacy, war, and trade failed and usually only further complicated an already complex situation. Ronda contends that American economic and diplomatic objectives were always uninformed and often simply wrong-headed; his research supports this view. But Lewis and Clark combined good sense and good luck to succeed as data gatherers for the nation.

Ronda's approach to his sources is rigorous, and his discussion demonstrates the most recent ethno-historical approaches. He has examined previously used data and from it has written an important and engaging study. The narrative is interesting, but, unfortunately, the University of Nebraska Press chose to publish this book with footnote-sized type.

Without a reading glass one has to be more than determined to struggle through the text.

ROGER L. NICHOLS
University of Arizona

CHARLES D. LOWERY. *James Barbour, a Jeffersonian Republican*. University: University of Alabama Press. 1984. Pp. xi, 320. \$32.50.

"James Barbour does not stand in the first rank of great men his age produced. But among the second rank, composed of individuals whose activities gave them national stature if not preeminence, he is entitled to a place of prominence" (p. 240). This estimate of Barbour by Charles D. Lowery is fair enough, and it is characteristic of Lowery's judicious treatment of his subject.

Barbour was an anomaly in the Virginia politics of the 1820s and 1830s. He was a pragmatist and a compromiser in a state that was becoming intellectually rigid and dogmatic. He was a nationalist in a society of provincials. He began his political career in 1798, a faithful adherent of Jefferson and states' rights. But, unlike many of his fellow Virginians, Barbour never made a fetish of the "principles of 1798." Experience in Congress, to which he was elected in 1803, broadened his horizons and tempered his politics. The War of 1812 made him a nationalist, and after the war he embraced the trinity of the new nationalism—bank, tariff, and internal improvements (though he waffled some on the last).

In the U.S. Senate, Barbour supported the Missouri Compromise, although he voted against the slavery restriction in order to maintain his standing in Virginia, and he worked hard to win acceptance of the compromise among Virginians. In the 1820s he befriended Henry Clay, endorsed the American system, and served as secretary of war to John Quincy Adams. In the 1830s he was one of the organizers of the Whig party in Virginia, living just long enough to share in the Whig triumph of 1840.

Barbour did more than buck the prevailing political climate of provincial Virginia. He blended his nationalism with a comprehensive philosophy of social reform. He agreed with Clay that government could be a positive force for economic development, and he thought it could serve social purposes as well. Barbour worked all his life to advance public education, both at the elementary and the university levels. He steered through Congress a charter for George Washington University in the District of Columbia and late in life campaigned unsuccessfully for an agricultural professorship and an experimental farm at the University of Virginia. He was unable to persuade Congress to make dueling a federal crime, but he did obtain a law abolishing imprison-

ment for debt in federal courts. His one shortcoming as a social reformer was slavery. Like Jefferson, he regarded slavery as an evil but could see no alternative. He joined the colonization movement and unsuccessfully urged his neighbor James Madison to free his slaves, but he never emancipated any of his own. He transmitted some of his liberal faith to his daughter, however, for in the 1850s she set free the slaves she inherited from him.

Barbour shared Jefferson's reverence for land, and he fully imbibed the Jeffersonian axiom that independent yeomen were the foundation of the republic. He invested the earnings of his law practice in land, eventually amassing an estate of more than twenty thousand acres. After he left political office in 1829, he became a full-time planter. He was both an experimental one and a successful one. He wrung a good profit from his plantation in all but the most adverse years, and he did so without selling any of his "surplus" slaves. Indeed, his labor force almost doubled itself (from 80 to 150) in the space of twenty-five years.

Barbour, then, was more than a prominent statesman of the second rank. He was a man of insight and social conscience, whose story enriches our concept of Jeffersonian democracy. Barbour's story deserved to be told, and Lowery has done a good job of telling it. His research is thorough, his writing lucid, and his conclusions judicious. My one cavil is that, in describing the Virginia landscape and agricultural methods in the 1830s, he would have benefited enormously from an acquaintance with the work of Carville Earle and historical geographers, who have modified substantially the picture drawn by European travelers of the day.

NORMAN K. RISJORD
University of Wisconsin

GOODWIN BERQUIST and PAUL C. BOWERS, JR. *The New Eden: James Kilbourne and the Development of Ohio*. Lanham, Md.: University Press of America. 1983. Pp. xvi, 289. Cloth \$27.75, paper \$13.75.

Goodwin Berquist and Paul C. Bowers, Jr., have written a good book about James Kilbourne, a prominent early Ohio businessman and politician. Like many state leaders, Kilbourne never played a major national role, but through his biography we can learn much about northwestern life—about politics, faith, and urban boosterism.

Kilbourne was an active Ohio town promoter. Born to a poor Connecticut farm family, he made useful contacts with important people and worked incessantly to develop a successful clothing mill. He became a Jeffersonian and an Episcopalian in a state where both groups were unpopular minorities, and, like many other Connecticut Jeffersonians, he even-

tually sold his property and moved to Ohio. There he founded the Episcopalian town of Worthington, took the lead in shaping Sandusky, created a complex of mills and outlet stores, and got elected to Congress. Ever the optimist, he tried to make Worthington the state capital and promoted a canal system that would make Sandusky the center of Ohio commerce. Rounding out a full life, he served as an Episcopal minister and as a college regent.

Larger forces destroyed these dreams. The panic of 1819 ruined his overextended business, and without wealth he lacked the political strength to create the Sandusky canal. After quarreling with his parishioners, he even lost his church. He still served periodically in the legislature, where he defended education and the Black Laws and condemned temperance and abolition. Lawsuits occupied his older years, and he died without wealth in 1850. Kilbourne's successes, flaws, and failures tell us much about Ohio's social and economic development.

The authors spent many years assembling this book. It is well written, fair, and lavishly documented. Biographies of representative figures are one good means toward achieving a greater understanding of a society, and in this sense Berquist and Bowers's study of James Kilbourne marks a welcome addition to our literature on the early Northwest.

JEFFREY P. BROWN
New Mexico State University

DANIEL FELLER. *The Public Lands in Jacksonian Politics*. Madison: University of Wisconsin Press. 1984. Pp. xvi, 264. \$29.50.

The ghost of Frederick Jackson Turner still seems to haunt the Madison campus of the University of Wisconsin. It is a good thing. Years ago Turner and his students emphasized the importance of sectionalism in early American history. They sometimes went too far in denying the relevance of political parties or in subsuming them under the blanket of sectional conflict, but, nonetheless, they identified geographical considerations as crucial to our understanding, especially of the 1820s. In this small, clearly written book Daniel Feller approaches the important question of land policy in an essentially Turnerian fashion, although he disputes some Turnerian stereotypes. Like the issues of internal improvements, Indian policy, and the tariff, land policy has received short shrift since Arthur Schlesinger, jr.'s, classic study put the bank issue at the center of Jacksonian politics. To a degree Feller accepts this, but he tries to fit both land policy and internal improvements into the pattern.

In the years after the War of 1812, the land issue was mainly one of the disposition of federal lands in

the "public land states" of the new West and the South. That provided a potential for a sectional alignment of national politics. Land policy raised a series of questions. Should the government sell land on credit? By 1820, in the midst of hard times, the government was owed millions of dollars. Congress, therefore, put an end to credit but struggled for a decade to deal with its creditors. Should the federal presence remain once statehood was established? The extreme states' rights element fought for cession of the lands to the states. They, along with others who desired to get the federal government off the states' backs, also proposed a graduation of land prices to speed up the process of transfer into private hands. The same forces pushed the idea of preemption, which would make it easier for squatters to purchase their farms. Should the national government manage what John Quincy Adams called this "vast public treasure" to encourage education and internal improvement through the distribution to the states of revenue from the sale of public lands? Those fearful of the federal behemoth looked askance at such proposals. The whole matter, as Feller correctly argues, was confused by the moralistic presentation of a false conflict between the "actual settlers" and the "speculators."

The combatants in each dispute manipulated these symbols so that through the 1820s and into the 1830s sectionalism—or what might better be seen as state interests—dominated the debates while politicians struggled to strike a balance of interests. By the mid-1830s, Feller argues, the issue was transformed from a sectional to a partisan one. "On the land issue, Old Hickory's second term had witnessed the crystallization of party positions along lines visible in 1833—the Whigs for distribution, the Democrats for preemption and, equivocally, for graduation" (p. 186). The compromise on the tariff and the Bank War served to transform the political situation into one of party conflict on a national basis. Feller specifically denies the old Turnerian idea of a South-West alliance in favor of another conception—the gathering of republican elements behind the party of Jackson. Since the Turnerians, historians have wrongly ignored the land issue; Feller has written a good book that expands and corrects their ideas. Unfortunately, his narrative has little connection to his hostile comments on "the new orthodoxy" of those emphasizing "the primacy of local over national and of ethnocultural over economic concerns" (p. xv).

WILLIAM G. SHADE
Lehigh University

MAURICE G. BAXTER. *One and Inseparable: Daniel Webster and the Union*. Cambridge: Belknap Press of Harvard University Press. 1984. Pp. vii, 646. \$25.00.

Maurice G. Baxter's biography of Daniel Webster is a work of prodigious and exacting scholarship. He has given us Webster the whole man, public and private, with all the foibles and virtues. The format is narrative and the style lucid, straightforward, and unadorned. For completeness, accuracy, and fairness this work has no peer, nor is it likely to have one in the foreseeable future.

Baxter clearly admires his subject, but Webster's shortcomings are amply documented: his vanity, his love of the good life, his relentless quest for money as well as his inability to manage it wisely. Treated fully for the first time are the details of his improvident investments (mostly in western lands), their disastrous failure, and his resultant dependency on the largesse of rich and well-placed friends. Baxter doubts that Webster would have behaved much differently on the issues without these favors and, given his New England base of power, he is probably right. But the extent of the godlike's greed is a telling comment on the man, as his dependency is on the class character of his politics.

The dominant (and for Baxter redeeming) motif of Webster's career in diplomacy, law, and politics is not class or section but nationalism. This Webster arrived at, we are shown, by way of a resolute New England sectionalism (a paradox worthy of resolution). Webster also fully adopted the aristocratic political style of New England Federalism, which involved him in yet another paradox. Like the old Federalists he was genuinely opposed to political parties; unlike them he was, by any measure, a professional politician. Yet for all his political skill he was never able to convince the American people of his greatness. All this is to say that Webster the nationalist was out of touch with the democratic course of national history.

His devotion to the Constitution, however, put him back in the mainstream of American nationalism. His impact on it in his great arguments before the Supreme Court—in such cases as *McCulloch v. Maryland*, *Dartmouth College v. Woodward*, and *Gibbons v. Ogden*—are analyzed masterfully by Baxter. So too is Webster's confrontation with the rising forces of antislavery in his state and section. Ironically, his willingness to sacrifice political popularity by choosing national union over morality on the slavery issue evinced a dedication to principle that was often absent in other areas of his life.

Webster, then, was a mythic figure who was not monolithic, a national symbol who was often at odds with national history. Still, no man of his age did more to fuse capitalism and nationalism into a cultural norm. Here, for better or worse, Webster was the "completest man" produced by his age. On this ground the man and the myth are identical.

KENT NEWMYER
University of Connecticut

DONALD B. COLE. *Martin Van Buren and the American Political System*. Princeton: Princeton University Press. 1984. Pp. xiii, 477. \$45.00.

If this book by Donald B. Cole had been published a year earlier, it might well have been the definitive study of Martin Van Buren. In 1983, however, John Niven's *Martin Van Buren: The Romantic Age of American Politics* appeared. Niven's work is also an old-fashioned, narrative, political biography, but one that is even more thorough than Cole's detailed book.

Born the year before the end of the revolution and dying a year after the beginning of the Civil War, Martin Van Buren matured during a transitional period of American politics and became a major figure among the second generation of political leaders. Lacking the heroic stature of an Andrew Jackson, the forensic skills of a Daniel Webster, or the powerful presence of a Henry Clay or John Calhoun, Van Buren nevertheless played the key role in the development of the modern two-party system, served admirably as Jackson's first secretary of state and second vice-president, and succeeded Jackson as president in 1837.

Contemporaries and historians alike have tended to view Van Buren as a "magician" and a "sly fox," an adroit political manipulator but at base a "mere politician." Both Cole and Niven modify this picture. Neither one denies that Van Buren was a consummately effective politician, yet both also find him to have been a man of true Jeffersonian Republican principles, although one who could on occasion bend ideology for political gain. Cole calls him "shrewd" but not "conniving" (p. 188). This seems a valid assumption. More questionable is Cole's claim that Van Buren was "the representative man of the age" (p. 254). A northerner born of poor yeoman stock, Van Buren moved from country to city and became a great success. Although such a career may have been "representative" of some Americans, it was clearly not true of the majority.

Cole is at his best in dealing with Van Buren's pre-presidential career. One gets an excellent sense of Van Buren's central role in the transition from factionalism to a modern two-party system during the Era of Good Feelings, first in New York State and then nationally. Cole also presents a marvelous picture of Van Buren's moderating role during Jackson's presidency and of the young Van Buren's close relationship with the Old Hero. Cole's coverage of Van Buren's presidency and later career, though adequate, is less satisfactory and breaks no new ground.

Cole's book, though well researched and nicely balanced between anecdote and analysis, has some major shortcomings. Throughout the book Cole writes of "Jacksonian democracy" without reference

to the nearly two generations of scholarly debate concerning the meaning (or non-meaning) of that term. Another problem is that Cole concentrates almost exclusively on the political issues, with only brief, superficial references to the momentous economic, social, and cultural changes that affected Van Buren and the political system. Cole also neither bothers with the questions that Lee Benson raised both about the identity of Van Buren's New York State constituents and why they voted as they did nor with recent analyses of voting patterns in state legislative bodies as an indicator of party formation.

Despite these deficiencies, Cole's and Niven's biographies of Van Buren present a more judicious, complete, and convincing account of the eighth president than any previous studies.

DOUGLAS T. MILLER

Michigan State University

ROYCE SHINGLETON. *Richard Peters: Champion of the New South*. Macon, Ga.: Mercer University Press. 1985. Pp. xiv, 258.

In this biography of Richard Peters, Royce Shingleton views the New South as "an article of faith, a vision of the future" (p. ix). Although Shingleton sees Henry W. Grady as the greatest symbol and publicist of the New South, he believes that Richard Peters was the major practitioner of the movement. Long before Grady was making his powerful addresses in the North concerning the New South, Peters was practicing the major tenets of the movement with his work in Georgia in the areas of transportation, urbanization, industrialization, and diversified farming.

Peters was born in 1810 in Germantown, Pennsylvania, of English-Scotch-Irish ancestry. His paternal grandfather was Richard Peters (1744-1828), a revolutionary patriot and later a federal district judge. After a few years of formal education the young Peters spent eighteen months at the Franklin Institute in Philadelphia. He briefly worked with the architect William Strickland and later was a civil engineer on railroads in New Jersey and Pennsylvania. In 1835 he moved to Georgia to be an assistant engineer under J. Edgar Thomson, the chief engineer of the newly organized Georgia Railroad. Soon Peters was made superintendent of the line and remained with the railroad until its completion to Marthasville, Georgia, in 1846. Shortly after leaving the Georgia Railroad, Peters established a stage line from Marthasville serving western Georgia and much of Alabama. Peters was responsible for changing the name of Marthasville to Atlanta, and he was an early "booster" of the town. He purchased much real estate there and in 1856 set up the largest flour mill south of Richmond. Earlier he had purchased

1,500 acres of farmland north of Atlanta in Gordon County. Peters sought to make the South less dependent on cotton and became known as a major exponent of scientific agriculture. He developed his land into a model plantation where he conducted extensive experiments with new crops and grasses and imported the best breeds of livestock from Europe.

As a conservative Whig Peters opposed secession, but when Georgia seceded he loyally supported the Confederacy. During the war he served the South as a civilian transportation agent and increased his wealth by participation in an active blockade-running company. When General Sherman burned Atlanta, Peters moved to Augusta but was among the first to return for the rebuilding of the city. Although never active in politics except at the city and county level, he managed to cushion the impact of Reconstruction on Atlanta. He played a major role in the removal of the state capital from Milledgeville to Atlanta. Peters was a promoter of a street railway in the city and in 1870 became one of the lessees and directors of the Western and Atlantic Railway. Throughout the postwar years he continued to be an active supporter of diversified agriculture and the introduction of industry into the South.

Shingleton has produced a lively and definitive biography of Richard Peters. The text is supplemented with more than a dozen contemporary illustrations and a brief bibliographical note. In his fully documented account of Peters's long and productive stay in the South, the author proves that Peters was a major champion of the New South. It may be harder to agree with the author's suggestion that "Peters was the greatest New South practitioner of all time" (p. 5). The fifty-four years that Peters lived in the South were spent almost entirely in Georgia and Atlanta. Certainly, Shingleton has written an excellent biography of a major figure in the long development of the New South.

JOHN F. STOVER
Purdue University

BURTON RAFFEL. *American Victorians: Explorations in Emotional History*. Hamden, Conn.: Archon. 1984. Pp. xvii, 191.

Burton Raffel's book offers brief studies of twelve figures, some of whom—such as Daniel Webster, James Fenimore Cooper, and Horace Greeley—are well known, whereas others—Louise Amelia Knapp Smith Clappe, William H. Brewer, and Susan Hale—are more or less obscure. Through the use of diaries, letters, autobiographies, and other personal records, Raffel's purpose is "to penetrate as deeply as I am able into the private worlds of these Victo-

rian Americans" in an effort to write "inner rather than external history" (p. xiii).

Raffel's view that "inner history" is somehow separable from "external history" is a major conceptual flaw in his book. He is reluctant to recognize the degree to which private perceptions and experiences reflect larger historical patterns: in his words, he does not wish "to explain anything larger than the person who happens to be the subject of my investigation" (p. xiii). But phenomena such as John Pintard's benevolent activism, Webster's work ethic, and John S. Wise's southern chivalric ideal cannot be fully understood without reference to the historical cultures that shaped their expression. If Raffel avoids moving outward from the lives of his subjects, he does not always "penetrate" very far into their "private worlds" either. His discussions of Thurlow Weed's political expertise, Cooper's ambivalence about being American, and Washington Irving's success at mirroring the interests of his contemporaries are not particularly revealing, in part because of Raffel's decision to write "emotional history," which explores only those feelings that are directly expressed, rather than psychohistory. Furthermore, Raffel's interest in "the contrast between the inner lives of those long-dead individuals and the inner lives of Americans today" (p. xiii) sometimes leads him into presentist judgments of his subjects, as when he charges Pintard with "religious fanaticism" for his belief that an outbreak of cholera was an expression of God's wrath at a guilty nation (p. 20) or marvels at the "singularity" of Greeley's abstinence from alcohol (p. 118). Along similar lines, Raffel's assessment of Hale as a "spinster" who was "distinctly sour, deeply unfulfilled" (p. 151) is not substantiated by his excerpts from the letters of this lively and careless social snob.

A professor of English, Raffel readily acknowledges, "I am not writing what is usually thought of as history" (p. xvi). Insofar as he avoids linking the private worlds of his subjects with larger historical patterns, his point is well taken. But in his preface Raffel suggests an argument that is historical, although he does not develop the idea in the body of his work: "Emotional states progress, and change, with time" (p. xv). If the nature of private experience does change over time, as much recent work in social and cultural history suggests, then the distinction Raffel has made between "inner" and "external" history may become increasingly difficult to maintain.

KAREN HALTTUNEN
Northwestern University

ELISABETH GRIFFITH. *In Her Own Right: The Life of Elizabeth Cady Stanton*. New York: Oxford University Press. 1984. Pp. xx, 268. \$17.95.

For many people, especially for women in nineteenth-century Victorian America, self-confidence is hard to come by, as is the kind of stamina and energy required to take a rebel stance. Yet Elizabeth Cady Stanton succeeded grandly, not only in personally rejecting contemporary norms of feminine propriety but also in launching an effective political campaign. That is not to say that she had no weaknesses. Elisabeth Griffith's biography bluntly shows that some "elitist, racist, [and] nativist" perspectives occasionally marred Cady Stanton's otherwise egalitarian ideals. Yet despite some disturbing limitations Cady Stanton still left us an impressive record. An abolitionist, a union organizer, and a major woman's movement theorist, she organized annual suffragist conventions, wrote countless articles and speeches, circulated petitions, collaborated on the multivolume *History of Woman's Suffrage*, wrote *The Woman's Bible*, and served as executive officer for national women's organizations for years.

Griffith's biography of Cady Stanton is spirited, informative, and thoroughly researched. Unlike earlier biographies, it probes personal as well as political dilemmas, albeit too abstractly in my opinion, and theoretically it is too narrowly based. Griffith uses Albert Bandura's "social learning theory" (p. 220) to explain the growth of Cady Stanton's strength and self-reliance, but I kept wondering about other theorists Griffith could have used as well. Nancy Chodorow's work on "the Family Structure and the Feminine Personality" (1974) could shed light on Cady Stanton's struggle, as could Carol Gilligan's affirming view of women's tendency to stress interrelational more than autonomous or self-reliant goals. Since Cady Stanton's life and work were explicitly so woman-centered, Griffith should have focused more directly on theorists probing patterns unique to women's lives.

Elizabeth Cady Stanton experienced directly many of "the wearisome cares" many women face—raising children, coping with unsupportive husbands, trying unsuccessfully to combine career and marriage. Griffith offers some starkly potent sketches here, which show why Cady Stanton's rage finally exploded into action, why she began responding less to other people's needs and caring less about their disapproval, and, finally, why she learned to know and trust herself. In her later years, Cady Stanton rigorously encouraged other women to develop self-reliance. But, more important, having personally fought a costly battle, she was campaigning for society itself to change.

MARY ARMFIELD HILL
Bucknell University

SARAH ELBERT. *A Hunger for Home: Louisa May Alcott and Little Women*. Philadelphia: Temple University Press. 1984. Pp. xiii, 278. \$27.95.

My childhood was happier because I believed in Jo March: I loved Marmee and wept at Beth's death, but it was rebellious and assertive Jo who spoke to me. With the appearance of Sarah Elbert's *A Hunger for Home: Louisa May Alcott and Little Women*, it is clearer exactly what that appeal constitutes and what it means as a cultural event.

Born into a New England reform family, Alcott, in novels and short stories, chronicled an entire history of nineteenth-century women's lives and work. Alcott's great contribution, Elbert persuasively asserts, was her ability to reach ordinary women with a full range of feminist concerns. Along with many liberal feminists of her generation, she linked her demand for democratic households to demands for woman's suffrage and a democratic society. An autobiographical writer, her fiction and her feminism flowed from and interacted with the chronology of her life. Her allegiance to a liberal woman's rights program never faltered, Elbert tells us, and her fiction made an important contribution to popular acceptance of liberal reform institutions.

Alcott accepted woman's traditional commitment to family and homelife, but at the same time she demanded individuality as her "natural right." She used her fiction to explore issues as they evolved in her own life: gender roles, filial loyalty, spinsterhood, marriage, motherhood, sexuality, and work. Not a biography in a usual sense, *A Hunger for Home* is cast in chronological mode, following Alcott's life and work in constant dialogue. For example, she joined the Civil War effort, using the opportunity for direct action as a nurse. During the war she made a connection between the rights of women and slaves, and she emerged confirmed in her earlier decision to remain single and use her energies as a writer to help project the form and content of a rational social order. The breakdown of family life, like the war between the states, Elbert says, was for Alcott a product of the inequality between men and women, black and white, rich and poor. She sought social laws of balance and harmony to replace unrestrained individualism on a personal and social level.

The sections on *Little Women*, surprisingly brief considering the book's title, carry out the developing thesis of the book in original and interesting ways. Elbert examines the subterranean messages below the conventional story of young girls learning self-sacrifice and overcoming faults on their way to becoming true women. It was, says Elbert, Alcott's domestic fiction, not her direct political activity in the woman's rights and suffrage movements, that constitutes her central feminist activity. She made the rights of women integral to her stories, above all to *Little Women*.

The ability to distinguish true love from romantic fancy is the central prerequisite for a woman's

growing up in that book. True love involves mutual self-sacrifice and self-control and a man who makes the household the center of his life and work. Romance, however, is inherently selfish, passionate, and unequal. And so Jo rejects the impetuous, sexual, boy-man suitor, Laurie, and chooses instead Frederick Baher, who is domestic, kind, and nurturing. Baher has the feminine attributes Louisa Alcott admired and hoped men could acquire in a rational feminist world. He is a committed reformer like Bronson Alcott, but he is a transformed, lovable Bronson. Together Jo and Frederick Baher embark on a model marriage and create a model society. Jo, the archetypical tomboy, attains the final stage of Alcott's sense of true womanhood. She has accepted maternal responsibility for the entire world. The conflict between feminist selfhood and domestic self-sacrifice is resolved by expanding the home to include work and expecting everyone, male and female, to hold equal responsibility for human nurturance.

Elbert has written a lively, intellectually provocative, and original book. It would have been improved with better organization, a clearer sense of a developing argument, and a better arrangement of the relationship between Alcott's life and her work, but she has given me my Jo in a serious and more meaningful way, and for that I am grateful and satisfied.

ANN J. LANE
Colgate University

CHARLES JOYNER. *Down by the Riverside: A South Carolina Slave Community*. (Blacks in the New World.) Urbana: University of Illinois Press. 1984. Pp. xxii, 345. \$24.95.

This sensitive and polished book explores slavery in the richest rice country of antebellum South Carolina—along the Waccamaw River, All Saints Parish, Georgetown District. Trained in folklore as well as history, Charles Joyner stands well equipped for a three-part task that begins with a particular black community; too often, he says, historians of slavery have drawn on evidence abstracted from different places and times. Secondly, employing the anthropologist's attention to culture and its total setting, he aims to study the slaves' inner lives—the feelings and thoughts behind the behavior that whites noticed. Joyner's third purpose is to examine—he says to explain—the development of Afro-American culture. He displays all the resourcefulness we would expect. Besides literary evidence and the stuff of more recent social history (court records, wills, estate inventories, vestry books, and federal census returns), he listens with a trained ear to Genievue W. Chandler's interviews of aged former slaves in

the 1930s. He uses recordings of black folksongs made in the locale at about the same time, discusses pieces of pottery (made by slaves, not Indians), and labors over Waccamaw folk tales, proverbs, and legends. Seeking tidbits that elderly slaves may have told their children and grandchildren but not white visitors, Joyner draws finally on his own field work among the descendants of All Saints slaves.

Joyner's findings fall squarely in the current consensus about the nature of the master-slave relationship and the vitality of the slave community: slaves were not merely property and were never completely powerless. The task system and the comparatively skilled work of rice growing, which some blacks knew from African experience and taught to whites, governed the life and labor of Waccamaw slaves and left them time to tend their own chores. Some slaves had other skills; Renty Tucker, the carpenter who built St. Mary's chapel on the Weston plantation, became well known for his craftsmanship. Blacks told stories about small animals that survived by pluck and guile. Only in their quarters, of course, did they spin tales of Slave John, who used similar tricks, including "lying" and "theft," to get along in bondage. Most masters took their paternal obligations seriously. Others did not. Beatings were not rare, and some overseers earned such enmity that freedmen in the spring of 1865 threatened their lives.

Joyner is at his best when tracing the persistence of African customs among the Waccamaw blacks and the interesting or exceptional details of their existence. Although organized by task, they worked together in the African manner. They ate noonday and evening meals communally and applied an African "culinary grammar" in preparing their rice. Like slaves along the Chesapeake, they had access to an enviable abundance of seafood—finfish and shellfish, but also turtle eggs, eel, and alligator. Joyner claims that masters and slaves often went after game together, a departure from custom that few slaves in the Old South could even imagine.

Joyner's book deserves more space. As interdisciplinary history it wins on a split decision. It commends the historian's reading in the sister sciences—Joyner perhaps "sees" more than he otherwise would—while also highlighting the inflated language and esoteric commonplaces of anthropology (on page 101 he announces, for example, that slave "hunting and fishing served at least two functions"—gathering food and sport). The book contains few surprises. But, especially when Joyner treats the fabricated integrity of the Gullah dialect, he convinces us that understanding the process of "Creolization" means studying it, where possible, in local detail.

ROBERT J. BRUGGER
Maryland Historical Society

MICHAEL P. JOHNSON and JAMES L. ROARK, editors. *No Chariot Let Down: Charleston's Free People of Color on the Eve of the Civil War*. Chapel Hill: University of North Carolina Press. 1984. Pp. xii, 174. \$16.95.

MICHAEL P. JOHNSON and JAMES L. ROARK. *Black Masters: A Free Family of Color in the Old South*. New York: W. W. Norton or Stoddart, Don Mills, Ontario. 1984. Pp. xvi, 422. \$22.50.

In the summer of 1935 in South Carolina three little girls found underneath a house a cardboard box containing thirty-seven letters that proved to be the only collection of sustained correspondence among free Afro-Americans in the late antebellum South. Painstakingly transcribed, fully annotated, and unobtrusively edited by Michael P. Johnson and James L. Roark in *No Chariot Let Down*, these letters allow a peerless view of a lost world. The editors did not stop with the letters, however. Johnson and Roark tracked down obscure references in hundreds of documents to produce *Black Masters*, a moving portrait of William Ellison and his world in the Old South.

William Ellison was the epitome of respectability. A wealthy South Carolina planter, he and his family lived in Stateburg in a house he had purchased from a former governor. He pursued a dual career in agriculture and manufacturing. In 1860 his sixty-three slaves produced eighty 400-pound bales of cotton on his 900-acre plantation. As one of the South's finest ginwrights, he made a major contribution to the prosperity of King Cotton. What was extraordinary about William Ellison, however, was that he was a wealthy planter who began life as a slave and purchased his freedom from his owner (perhaps his father or half-brother) at the age of twenty-six.

Ellison considered himself a "freed yellow man"—one who was "brown" or "colored"—not a black man. Despite his economic achievement the Ellisons, like other free persons of color in South Carolina, were subject to numerous discriminations and prohibitions. For violations they could be sold to the highest bidder. A reputation for "respectability," for being above reproach in white eyes, was essential. But the law did afford them crucial protection to life, liberty, and property that was denied to slaves. For example, they could sue to enforce contracts for work or payment, as William Ellison did on more than one occasion.

By 1820 Ellison had already begun to buy slaves of his own. In a slave society what better evidence of "respectability" could there be? As he brought his sons into his cotton gin business, he taught them to observe the subtle rituals of racial etiquette, to be deferential but not obsequious, to be ingratiating but not groveling. His own mix of self-assertion and

silent accommodation paid off in cash and respect, reinforcing the fragile foundations of his family's freedom.

All four of William Ellison's children married into Charleston's free mulatto elite, the "brown aristocracy." It was a "working aristocracy" of skilled tradesmen—tailors, carpenters, millwrights—whose wealth was largely in slaves and urban real estate. Over the centuries they had created for themselves a middle ground between white and black. Within the social space it provided, they wove the strands of a cohesive community, depending on the white aristocrats—their customers, fellow parishoners, patrons, and sometimes kin—to help them defend the niche they had carved out.

Johnson and Roark are at their best in their subtly argued analysis of the crisis of survival for free persons of color in the summer of 1860. In the mounting national tension, Charleston's white mechanics demanded that the middle ground—their acute competition—be eliminated. As the crisis deepened, free people of color were pushed inexorably toward slavery. Most of those actually enslaved were poor free blacks, but even the "brown aristocracy" had reason to fear the loss of their freedom. This "all-out assault on free Negroes," note the authors in a chilling analogy, "amounted to an antebellum *Krystalnacht*" (*Black Masters*, p. 237). As the foundations of freedom began to crumble, more than a thousand refugees fled to the free states and abroad. Slaves might sing "Swing Low, Sweet Chariot," but Ellison's son-in-law urged emigration rather than waiting for "the working of a miracle by having a Chariot let down to convey us away." "It is better to make a sacrifice now," he wrote from Charleston, "than to wait to be sacrificed *our selves*" (*No Chariot Let Down*, pp. 101, 147). Ellison and his family did not join the exodus, although he did send his grandchildren safely away to a Philadelphia boarding school. After the Civil War the Ellisons no longer enjoyed their special status and had to adjust to a new world, a world with no middle ground, one that knew only black and white.

No Chariot Let Down is the more focused of the two books, concentrating on Charleston's free-born elite in the crisis of 1860. The editors supply an elegantly written introduction and enough annotation to make the letters comprehensible but otherwise let the documents speak for themselves. *Black Masters* is more interpretive. It aspires to be a history of the Ellison family, but it is "both more and less than that" (p. xi). It is more because it not only chronicles—often brilliantly—the rise of William Ellison and his family but also analyzes how their lives were bound up with those of other free Afro-Americans and how they affected and were affected by the shifting politics of South Carolina on the eve of secession. It is less, however, because too much

"remains unknown and unknowable" (p. xi). *Black Masters*—despite exhaustive research in manuscript census returns, court records, deeds, wills, tax returns, parish registers, town ordinances, white correspondence, even tombstone inscriptions—rests on inference to a significantly higher degree than most books. Its authors are too honest to try to disguise inference as fact: the characteristic sentence in *Black Masters* begins, "Ellison may have" or "Ellison must have." In some cases—notably, in their discussion of Ellison's apparent practice of selling little girls—inference is built on inference. "Sensitive to public opinion," they write, "he probably did not stray too far from prevailing practices" (p. 136). The inference regarding prevailing practices is built on the inference that Ellison was sensitive to public opinion (because, they infer, he had to be). Although other inferences might be drawn, those of the authors are sensible, judicious, full of stimulating suggestions, and consonant with the known evidence. By providing vividly interesting insights into the world of South Carolina's free mulatto elite, *No Chariot Let Down* and *Black Masters* make original and important contributions to American social history.

CHARLES JOYNER
Coastal Carolina College
University of South Carolina

FRANK L. KLEMENT. *Dark Lanterns: Secret Political Societies, Conspiracies, and Treason Trials in the Civil War*. Baton Rouge: Louisiana State University Press. 1984. Pp. xiii, 263. \$25.00.

This is a cautionary study that should be taken seriously by historians and those interested in politics. After completing Frank L. Klement's book, the reader should take a vow never to pass on a rumor. In his introduction Klement quotes Shakespeare: "Rumor is a pipe, blown by surmises, jealousies, conjectures" (p. 2). The thesis of this book is that from the pipe of rumor bubbles floated into the air containing little more substance than those from a child's pipe. Yet those bubbles were seen by a people at war as having concrete form and have been accepted by historians as fact rather than as rumor and conjecture.

Klement is the historian who has transformed the image of wartime Democrats and their activities. Two carefully researched books, *Copperheads in the Middle West* (1960) and his biography of Clement Vallandigham (1970), have been well received. Some historians believe that Klement absolved the Copperheads of too much but that his picture of the secret societies was more accurate. *Dark Lanterns* focuses on those secret societies in the same careful manner, and Klement makes his case well. His

sketch of the societies in *Copperheads in the Middle West* has become a finished canvas in *Dark Lanterns*.

The three Democratic societies were the Knights of the Golden Circle (KGC), the Order of the American Knights (OAK), and the Sons of Liberty. For reasons of personal and political profit Republicans (often members of the Union League) portrayed them as organizations with five hundred thousand members who were going to burn New York, treat Union Leaguers to a St. Bartholomew's Day Massacre, and seize the polls on election day, 1864. Reports claimed that William Yancey initiated George McClellan into the KGC and that the OAK password was "nu-oh-lac"—Calhoun spelled backwards.

KGC organizer George Bickley and OAK leader Phineas C. Wright were shabby men with grandiose schemes who threatened no one. Bickley attracted the attention of Illinoisan Joseph K. C. Forrest who, for Republican gain, spread stories of KGC power. Wright's "dead" organization was transformed from a "molehill into a mountain" by Colonel John P. Sanderson, a henchman of Simon Cameron who wanted promotion and Republican victory. The Sons of Liberty was a slightly more serious group whose leader, H. H. Dodd, was a little less of a charlatan.

After tracing the development of the societies, Klement explores the Indianapolis treason trials and the Cincinnati treason trial. All, he writes, were politically timed and motivated and filled with circumstantial evidence and civil rights violations of men whose crimes, for the most part, were not treason but excessive partisanship. Klement's portrayal of those on trial is balanced, presenting many as stupid, arrogant, and naive to the forces at work in the land.

He concludes that the societies were no threat to the Union. In eleven pages near the end of the book, he traces the continued circulation of these rumors by historians and comments, "Legends and myths nurtured by nationalism and based upon wartime political propaganda remain a part of consensus history" (p. 244). Klement's book not only counters those legends and myths but also stands as a warning to historians and to nations at war.

JAMES P. JONES
Florida State University

JOEL WILLIAMSON. *The Crucible of Race: Black-White Relations in the American South since Emancipation*. New York: Oxford University Press. 1984. Pp. xviii, 561. \$25.00.

This is one of the most provocative books on race in America written in the past ten years. In one sense, the title is not altogether accurate; this is less the

history of black-white relations in the South than it is an audacious, and often moving, account of one white southern historian's attempt to unravel the complex history of white attitudes and ideas about race.

In their treatment of their former slaves white southerners moved from violence and repression in the post-Civil War years to the systematic use of the most sadistic and barbaric forms of public torture, mutilation, and (often as a merciful climax) hanging. Why? And why did white southerners insist on the dehumanization of the black men and women in their midst with sadistically ingenious rituals of humiliation, daily ceremonies of debasement that the clinical term "segregation" can never convey? To these questions Joel Williamson offers no easy or conclusive answers, but he has created a new synthesis of the history of post-Civil War race relations.

At the core of Williamson's book is his description and analysis of three traditions in white attitudes toward race: liberalism, conservatism, and a virulently antiblack form of radicalism. White liberals were few and ineffective in the postwar South; the conservatives—heirs in some respects to antebellum paternalism—shaped southern race relations. But in the 1890s radicals became the dominant force in the region. A patronizing white conservative who emphasized racial tranquility was no match for a radical like novelist Thomas Dixon, who, in *The Leopard's Spots* (1902) and *The Clansmen* (1905), recorded his nightmarish ravings of black bestiality and his florid defense of lynch law.

Williamson's typology of white attitudes is much more subtle than this brief summary implies. It is further strengthened by his effort to place the radicals in the context of the depression of the 1890s. As white men found themselves increasingly unable to provide for their families (and particularly for their wives), they compensated for their inadequacy by protecting "their" women from an even greater threat than deprivation: the loss of purity at the hands of "black rapists." The intense conjunction of sexual and racial attitudes in the midst of economic deprivation laid the foundation for the nightmare of racial violence and oppression that peaked in the 1890s.

Any brief review runs the risk of caricaturing Williamson's insights on this and dozens of related subjects. He suggests that the rise of a black counterculture was a product of the disintegration of black life in the face of white radical onslaughts. He sympathetically describes the frustrations of black intellectuals caught in a seamless web of repression. He tells of the tragic contribution of white southern women like Rebecca Felton to the radical catechism.

Almost anyone who reads this book will be challenged and sometimes provoked into dissent. That response is a tribute to the compelling honesty of a

historian willing to tell the truth as he sees it, even when it challenges our conventionally fashionable analysis.

DAN T. CARTER
Emory University

WILLIAM R. BROCK. *Investigation and Responsibility: Public Responsibility in the United States, 1865–1900*. New York: Cambridge University Press. 1984. Pp. viii, 280. \$39.50.

Most scholars, at one time or another, come across a book they wish they had written. This one is mine; oh, how I wish I had written this work—a highly researched yet concise, clearly reasoned, and exceedingly well-written monograph that convincingly argues that in the latter half of the nineteenth century the American states assumed a great deal of responsibility for the welfare of their inhabitants and, in the process, laid the basis for the later welfare state. Why it took so long for someone to say what now seems so obvious but what heretofore had not been said, let alone so impressively documented, is somewhat of a mystery. But, whatever the reason, we are indebted to William R. Brock, the distinguished and erudite British historian of America, for this fine work.

A long time ago, of course—since at least 1955 and the publication of Robert Lively's brilliant review essay on "the American system"—students of American economic history put to rest the myth of laissez faire in the nineteenth century. The belief, however, was that the states and the federal government provided all sorts of assistance to the business classes for economic development but that in matters of social welfare they did not play a significant role until the twentieth century, first during the Progressive era and then during the New Deal. That myth now also is shattered, at least as far as the states are concerned. With regard to social policy and action, the so-called Gilded Age was "not an age apart but the beginning of the epoch in which we now live" (p. vii). It was at that time, not later, that a substantial number of Americans came to believe that the duty of the state was to remedy those defects in society that individuals alone could not overcome. And these Americans did something about it; namely, they created a number of agencies to regulate and improve the social and economic environment.

Brock begins his work with chapters on public responsibility, in theory and practice, and the law in late nineteenth-century America. (Among other things, the latter includes an interesting reassessment of judicial opinion in which he maintains that the courts sought to facilitate, not curb, increased government intervention and responsibility for pub-

lic welfare.) He concludes the book with an analysis of the way in which experiences in the states influenced the later development of national policy, and not always for the better, because advances in the states sometimes made it more difficult to enlarge federal responsibility. The heart of the study, however, lies in the four chapters on those state agencies created all over America in the years after the Civil War to deal with increasing deprivation, growing health hazards, deteriorating working conditions, and untrammelled economic power—boards of state charities, boards of public health, bureaus of labor statistics, and railroad commissions. Such agencies not only were created but also were effectively administered; although their progress was uneven, it was steady and significant. Thanks to their efforts, treatment of the unfortunate improved, the health of the public was safeguarded, the conditions of work were enhanced, and the economic power of the well-to-do was increasingly regulated. In short, public responsibility for remedying the defects of the social and economic order, now hallmarks of the modern era, actually began at the state level between the Civil War and the end of the nineteenth century, not in the twentieth.

Perhaps one day Brock or someone else will write a companion volume on the federal government during that still-much-maligned period in American history; I think it can be done. Meanwhile, this book is mandatory reading not only for students of social policy but also for all those interested in nineteenth- and twentieth-century American history.

WALTER I. TRATTNER
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NANCY TOMES. *A Generous Confidence: Thomas Story Kirkbride and the Art of Asylum-Keeping, 1840–1883*. (Cambridge History of Medicine.) New York: Cambridge University Press. 1984. Pp. xv, 387. \$39.50.

The new history of psychiatry and mental illness that appeared in the 1950s and 1960s was based on new research into original sources: hospital reports and records, contemporary journals and newspapers, government publications, and diaries, letters, and other manuscripts. It presented a positive view of the short-lived, early nineteenth-century reform of hospital care of the mentally ill. Historians saw as progressive and humane the new system of moral treatment, a form of milieu therapy intended to cure insanity through the creation of a benevolent environment and the exercise of moral suasion. In the 1970s other scholars discovered Karl Marx and the class struggle and concluded that the object of mental hospitals had always been social control in

the interest of preserving capitalism rather than of curing mental illness or offering humane care. In this revision of the earlier revisionism, ideology explicitly predominated; evidence took on importance mainly as illustration. In the 1980s the inadequacy of this almost exclusive reliance on ideology is apparent and interest in careful research is growing.

This current trend toward combining the skepticism of the 1970s (albeit modified) with research in the sources is apparent in Nancy Tomes's valuable work, which contributes significantly to our understanding of moral treatment as implemented by one of the most respected hospital superintendents of his day, Thomas Story Kirkbride, in one of the most prestigious mental hospitals in North America, the Pennsylvania Hospital for the Insane at Philadelphia. Tomes's conclusion that in his attention to architecture Kirkbride was creating the conditions necessary to practice moral treatment is contrary to the usual view that he cared more for plumbing than for patients. Construction of attractive buildings also won the financial support of patients' families, something that private institutions always needed. Another important point that Tomes convincingly argues is that Kirkbride's stress on a small hospital population enabled him to become intimate with patients (a tenet of moral treatment) and to provide better custodial care than is available anywhere today. If other superintendents, Tomes argues, had followed his practice of admitting patients from all socioeconomic strata into the hospital, the fatal segregation of upper and lower classes into separate and unequal institutions might have been less common (if not altogether avoided) and moral treatment might have had a longer life span.

Tomes does not ignore the contradictions and problems in Kirkbride's philosophy and practice of moral treatment, particularly the fact that many of the patients in his hospital were involuntarily confined, a situation that inhibited successful therapeutic treatment. Overall, her evaluation of Kirkbride is positive, though somewhat reluctantly so, and she is perhaps naive in criticizing him for pursuing his own self-interest in accepting the superintendency of the Pennsylvania Hospital for the Insane. Tomes is on the whole fair in her judgments and careful with her evidence. She has produced a fine work, valuable to specialists and of general interest to social historians and historians of medicine as a thoughtful study of a creative man and an important social and medical institution.

NORMAN DAIN
Rutgers University

WILLIAM H. LECKIE and SHIRLEY A. LECKIE. *Unlikely Warriors: General Benjamin H. Grierson and His Family*.

Norman: University of Oklahoma Press. 1984. Pp. xv, 368.

William H. Leckie and Shirley A. Leckie have produced a book different in its coverage from that of the usual military biography. Their work encompasses the army career of General Benjamin Grierson in the Civil War and on the American southwestern frontier. It also covers the life and times of three generations of the Grierson and Kirk families in intimate detail. Indeed, social history, not military, dominates the volume.

Benjamin Grierson was a talented musician and plied his trade as a bandleader, composer, and teacher. He married Alice Kirk in 1854 and sought to supplement his income in various mercantile enterprises. By 1861 he was heavily in debt and gladly joined an Illinois infantry regiment to help quell the rise of southern nationalism and to escape his financial problems.

Grierson quickly experienced success in the Union Army during the Civil War. He gained promotions in the Sixth Illinois Cavalry and served with Generals U. S. Grant and William T. Sherman in Tennessee and Mississippi. His greatest claim to fame rests on his daring sixteen-day, six-hundred-mile sweep through the state of Mississippi. His exploit brought fear and devastation to the heart of the Confederacy and proved a helpful diversion to Grant's campaign at Vicksburg. The next year he led another successful raid into Mississippi, and at war's end he was leading his cavalry division in Alabama. The major general had indeed earned recognition as one of many national heroes.

In the postwar period Grierson remained in the army and served as colonel of the newly organized Tenth Cavalry, a unit made up of white officers and black soldiers. His duty carried him to remote posts in the arid Southwest where he served until 1890. Although he led his regiment in the Victorio War of 1880, he preferred more humanitarian methods in dealing with Indians.

As important as military matters were, the Griersons also struggled with domestic concerns. Ben and Alice produced seven children during their stormy married life. Four survived to adulthood and two of those suffered from insanity, a genetic condition found among members of the Kirk family. Financial concerns also nagged the family for years because the Griersons had to support several members of their extended family. The results of physical and financial setbacks and the drudgery of life at isolated frontier posts resulted in a bleak existence and marital problems between Alice and Ben. Only the couple's enduring strength of will saw them through these various ordeals.

This is a handsome volume that includes numerous photos illustrating the various stages in the

Grierson family development. The only disappointment in the book is the lack of analysis concerning Grierson's service in the Southwest with his black troopers, a topic that Leckie covered in his earlier work, *The Buffalo Soldiers*. The research for *Unlikely Warriors* is impressive, and the authors have not only tapped the federal records and newspaper sources but also mined the voluminous correspondence of the various family members. The result is a definitive biography that contributes a vivid picture of the Grierson family and of the stark reality of American life in the last half of the nineteenth century.

JOHN W. BAILEY
Carthage College

SANDRA L. CADWALADER and VINE DELORIA, JR., editors. *The Aggressions of Civilization: Federal Indian Policy since the 1880s*. Philadelphia: Temple University Press. 1984. Pp. xvi, 258. \$34.95.

In 1885, three years after its founding, the Indian Rights Association (IRA) declared, "The Indian as a savage member of a tribal organization cannot survive, ought not to survive, the aggressions of civilization." It was a statement reflecting the movement for reform in Indian policy that sought to detribalize the Indians and assimilate them completely as individuals into white society. In 1982, on the centenary of the IRA's founding, the association hosted a conference to examine how the Indians had reacted to the "aggressions of civilization" during the past century. The papers in this small volume are selections from those given at the conference.

Like essays in most such collections, these are uneven in quality and somewhat repetitious. Yet as a whole they are of considerable significance. They show that Indian tribal groups have survived and have reestablished political and cultural autonomy. The book demonstrates as well the maturity that Indian studies dealing with twentieth-century history have reached. Historians and anthropologists—Indian and non-Indian alike—are able to investigate and analyze Indian policy without notable rancor or emotion. The result is a new understanding of the Indians' situation in the United States, a foundation on which realistic future policy can be built.

Some of the papers pay special attention to the role of the IRA. Vine Deloria, Jr., opens the volume with a brief appraisal of the association, which is remarkably benign considering that until well into the twentieth century the IRA was one of civilization's chief aggressors against Indian tribal society. Daniel M. Strausfeld considers the conflict between the IRA and the Bureau of Indian Affairs over Indian dances in the 1920s and over the Wheeler-Howard Act in the 1930s, and Ann Laquer Estlin contributes a valuable study on *Lone Wolf v. Hitchcock*

(1903), in which the IRA unsuccessfully attempted to protect the Indian lands from forcible allotment.

The major essays, which give some unity to the book, survey disparate aspects of Indian history since the 1880s. James E. Officer treats the evolution of the Bureau of Indian Affairs (unfortunately, largely in a commissioner-by-commissioner chronological account). Deloria knowledgeably describes the role of Congress and the course of legislation dealing with Indians. Alvin J. Zions surveys Supreme Court cases, with emphasis on the issue of Indian tribal sovereignty. And Robert T. Coulter and Steven M. Tullberg jointly provide a brief history of the policy affecting Indian land rights. Theirs is the only essay in the book that seems to be more advocacy of a particular position than a balanced history and appraisal. Their view of the federal government's trust responsibility for Indian resources—which they call “racial discrimination and boundless United States power disguised as moral and legal duty” (p. 198)—contrasts strangely with the current desire of Indians to maintain and expand the trust relationship and their fear that that relationship and the benefits it brings to Indians might be terminated. The collection is rounded out by a wide-ranging essay of Wilcomb E. Washburn's, which notes the IRA's role in arousing moral outrage over the conditions of the Indians, and an overly harsh criticism of the Indian Reorganization Act of 1934 by Laurence M. Hauptman.

FRANCIS PAUL PRUCHA
Marquette University

ANNETTE ATKINS. *Harvest of Grief: Grasshopper Plagues and Public Assistance in Minnesota, 1873–78*. St. Paul: Minnesota Historical Society. 1984. Pp. 147. \$12.95.

In this book Annette Atkins attempts to do what Donald Worster did so successfully in *The Dust Bowl*—to illuminate social values by focusing on an environmental crisis and the public response to it. Atkins succeeds in advancing our knowledge of the grasshopper plague that struck western farmers during the 1870s and the measures taken to assist its victims, but she fails to offer important insights about social values.

Severe infestations of Rocky Mountain locusts, popularly known as grasshoppers, struck much of the Great Plains and the western Midwest for several years, beginning in 1873. In combination with the agricultural depression of the 1870s, this invasion spelled doom for many debt-ridden farmers. It also called forth a many-faceted and uneven relief effort—involving county, state, and federal governments, the army, and private trade, civic, agricultural, and charitable groups—as well as a concerted

scientific endeavor to prevent comparable depredations in the future.

In an interesting and sometimes fascinating fashion, Atkins tells the story of the grasshopper plague and public relief in Minnesota, with special attention to the southwestern and south central portions of the state. The locusts appeared first in 1873, and for the next five seasons they damaged crops in rather a capricious fashion, stripping some fields and leaving others untouched. In 1877, following a day of prayer proclaimed by Governor John S. Pillsbury, the newly hatched grasshoppers did little damage, and the next year they did not return to the state. Relief for victims, from private and public sources, was as uneven as the devastation wrought by the pests, but it generally declined in adequacy as the crisis continued.

There is much of value in Atkins's narrative, which is enhanced by helpful maps and well-chosen photographs and illustrations. But Atkins's sympathy with the farmers victimized by the catastrophe, though generally admirable, clouds her judgments and misshapes her interpretations. To Atkins, all of the recipients of aid were worthy and hard-working farmers in the Jeffersonian agrarian mold, whereas the dispensers of aid—particularly Pillsbury—were penurious, self-righteous people who suspected that those who needed help were unworthy of receiving it. These images and the assumptions that underpin them oversimplify complexities of human motivation and behavior and ignore the realities of an intricate situation.

On this flawed interpretive structure Atkins constructs the dubious thesis that the governmental response to the victims of the infestation showed that Americans equated human value with money and assumed that those without wealth were morally flawed and unworthy. Atkins's explanation of the nature and alteration of social values is one-dimensional and unsophisticated, and her comparison of the response to the grasshopper sufferers with the response to the victims of the Chicago Fire—a very different type of disaster—damages her case rather than supporting it.

This book increases our knowledge of an important environmental phenomenon and the reaction to it. Unfortunately, it fails to advance our knowledge of nineteenth-century American social values.

DAVID B. DANBOM
North Dakota State University

HELEN LEFKOWITZ HOROWITZ. *Alma Mater: Design and Experience in the Women's Colleges from Their Nineteenth-Century Beginnings to the 1930s*. (Borzoi.) New York: Alfred A. Knopf or Random House of Canada, Toronto. 1984. Pp. xxii, 420. \$25.00.

This is an ambitious book that explores the subtle interactions of people and environment. Helen Lefkowitz Horowitz draws the reader's attention outwards, linking behavior to architecture. She explains persuasively how a person's experience is conditioned by the physical as well as the organizational structure of an institution. Her book is generative, suggesting questions for future research: What impact may the diversity of architecture in colleges and universities today have on the intellectual experience of undergraduate and graduate students? How have architecture and cultural values interacted at the Ivy League colleges for men? How similar were the college experiences of a man and a woman at the same coeducational college, where each lived in a dormitory designed for gender-specific social life and each attended classes in buildings designed for gender-specific intellectual activities?

In examining the Seven Sisters colleges, Horowitz outlines a progression of architectural design: from Mount Holyoke's original large-scale house, where all activities took place and faculty and students lived together, to Bryn Mawr's Gothic academic and residential buildings that mirrored those of the men's colleges, with separate houses for the president, dean, and faculty. Horowitz describes the architectural changes that took place on each campus and the influence that one college had on another as new buildings were commissioned because of gifts, increased enrollments, or the destruction of old buildings by fire. She discusses how the gender-role beliefs of the colleges' founders, trustees, faculty, alumnae, and architects influenced the design of each building and how these beliefs changed over time. In a final chapter Horowitz examines the impact that beliefs about gender roles had on the architecture and organization of three twentieth-century colleges for women: Sarah Lawrence, Bennington, and Scripps.

Horowitz perceives architecture as an expression of cultural values, which, in the case of the women's colleges, were those values that defined women's roles in society. At the same time, Horowitz recognizes that cultural values are themselves affected by architecture. She suggests, for instance, that Matthew Vassar's intentions were subverted by the very structure he created. The main building at his college, with its suites of rooms for students, provided an atmosphere of privacy where a separate women's culture flourished, the very development Vassar intended to prevent.

Using insights from feminist scholarship in several fields, Horowitz explores the growth of self-defined student life and the gradual separation of women faculty from students. Her discussion of the way that the demand for academic freedom grew with the faculty's ability to live separately from the

students provides a new focus on the development of professionalism. Similarly, her descriptions of student life at Barnard and Radcliffe provide new insights into the role of the urban university.

The book would have benefited from a tighter structure, more balance between cultural and architectural issues, more space for the twentieth-century colleges, and the elimination of small but significant errors. Nonetheless, Horowitz's analysis of the interactions of people and environment in the women's colleges is an important contribution to social history and to the history of higher education in the United States.

JOAN N. BURSTYN
Rutgers University

MATTHEW EDEL *et al.* *Shaky Palaces: Homeownership and Social Mobility in Boston's Suburbanization*. (Columbia History of Urban Life.) New York: Columbia University Press. 1984. Pp. xxiv, 459. Cloth \$40.00, paper \$18.00.

Urban historians have long argued that American big cities grew by accretions at their edges. They have also contended that after the mid-nineteenth century this process left deteriorated neighborhoods of old and cheap housing in the urban core, produced on successive peripheries more pleasant neighborhoods of new and more expensive housing, and deposited between the inner and outer city successive zones of used and moderately priced housing that filtered down to those moving out from the center. Matthew Edel *et al.* have not questioned this conventional wisdom, but they have analyzed the elements of the process and its consequences in unconventional ways.

Shaky Palaces examines three centuries of Boston's growth to make two central historical points and draw a major conclusion about their meaning for our times. First, homeownership and suburbanization have repeatedly entrapped workers because the process of neighborhood deterioration devalued the investment of individuals in better homes and frustrated homeowners' chances for real social mobility through capital acquisition. At the same time, however, suburbanization buttressed the capitalist status quo. Because homeownership and suburbanization improved short-run living conditions they seemed, at least temporarily, a sign that the capitalistic system enhanced the standing of the working class.

Theoretically, then, only the end of suburbanization would create a profound crisis of confidence in the system among low and moderate income groups. That, in turn, would provide a golden opportunity for noncapitalist activists to channel frustrated homeowners into political coalitions for a potentially revolutionary change in the system. The

authors believe such a crisis is upon us, although they remind us that the outcome is not inevitable. Capitalists may contain discontent by repression or discover a moral alternative to the suburb. And anticapitalists may dissipate their opportunity, for their ignorance of the historical opiate of suburbanization may lead them to overlook the crisis and miss their grand chance to devise effective democratic strategies of withdrawal and transformation.

Thus, this book is an undisguised tract,³ but not a crude one. It presents intriguing methods for measuring individual and intergenerational social mobility and changing land and housing values. It also makes interesting and provocative suggestions about how suburbanization happened. The authors contend, for example, that the American commitment to homeownership and suburbanization stemmed from late nineteenth-century urban discontent and represented a class compromise: taxes and government expenditures were used to underwrite suburbanization as a conscious alternative to shanties, company towns, model tenements, and massive public housing as solutions to the problem of intolerable working-class housing in intolerable neighborhoods. They argue, too, that suburbanization undermined class consciousness generally as a significant political factor and hypothesize specifically that it killed the Socialist party. They also stress the roles of land subdividers, housing and industrial developers, and government in encouraging and shaping suburbanization and therefore in imposing both constraints on individual housing choices and inequities across the metropolitan mosaic of social and ethnic neighborhoods and tax districts. Along the way, moreover, one finds tantalizing vignettes, including a reassessment of Marxian analyses of homeownership and its consequences, a critique of simple-minded uses of the idea of social control, a class analysis of the community defense and tax-cutting episodes characteristic of the 1960s and 1970s, commentary on other devalued assets such as education, and a brief discussion of strategies for the future.

This is a compact volume, rendered more difficult to read by an untidy organizational scheme that eschews a consistently chronological cadence. But it is worth the effort and should be useful for those concerned with our urban past as well as those worried about the nation's future. Each reader, of course, will decide how to use it.

ZANE L. MILLER
University of Cincinnati

CARL S. SMITH. *Chicago and the American Literary Imagination, 1880-1920*. Chicago: University of Chicago Press. 1984. Pp. xiv, 232. Cloth \$26.00, paper \$14.00.

Fiction is out of favor as a historical source. Many historians scoff at the notion that novels can reveal anything about historical actualities; others dismiss imaginative literature as an "elite source." In part, these are understandable reactions against the misuse of literary evidence. Historians have too often assumed that cultural artifacts offer a transparent reflection of an unproblematic "reality." Under the impact of semiotics, literalism has recently waned, and historians have begun to realize that works of fiction constitute symbolic universes with their own conventions and inner logic. Contrary to semiotic dogma, however, those conventions and that logic are entangled with the details of history and biography. By acknowledging the peculiar epistemological status of fiction, historians can renew their exploration of a rich territory.

Carl S. Smith has staked his claim to that territory with admirable directness. He does not treat literature as a window onto the world or as a self-referential object. Focusing on the interaction between authorial consciousness and historical circumstance, this lucid monograph surveys the efforts of major writers to find expressive forms adequate to their experience of Chicago during its most frenetic period of expansion. Smith discusses the fiction of Theodore Dreiser, Henry Blake Fuller, Sherwood Anderson, Upton Sinclair, Frank Norris, and other major novelists as well as the nonfiction of Jane Addams and Louis Sullivan. Nearly all these authors imagined the city as a kind of laboratory where the veils of tradition and gentility were torn away and one could clearly view the workings of industrial capitalism. And, nearly always, the writers' reaction to the city was divided or ambivalent. For every Addams who shuddered at its brutalities, a Norris celebrated its crude vitality—even as he showed its business titans brought low by their own hubris. Smith traces not only the writers' response to exploitative economic relations, socially or artistically ambitious women, and empire-building capitalists but also their attempts to represent the physical embodiments of Chicago's dynamism and danger: the railroad, the skyscraper, the stockyards. Throughout, Smith identifies the writers' central concern: how does one make art out of such raw materials?

Asking that question too insistently, Smith overlooks others. He neglects the spatial dimensions of the problems Chicago posed. As William R. Taylor has argued with respect to New York City, Progressive urban planning not only reshaped public space but also redefined "the public" as a mobile, manipulable crowd. Chicago's towering train terminals may have been of a piece with the Chicago Plan for urban redevelopment: both helped create a monumental public order that would promote efficient management of a faceless public.

A larger neglected question involves the emergent culture of industrial capitalism. Smith assumes but does not analyze a "prevailing ethos of materialism" (p. 79) in Chicago. He does not discuss some of the tensions and transformations within that ethos: Dreiser's Carrie Meeber and Clyde Griffiths, for example, suggest the emergence of a new, more elusive sense of selfhood characterized by perpetually swelling desires and sustained through the assemblage and reassemblage of emblematic commodities. Perhaps Smith could have addressed such issues more successfully if he had been more sensitive to language itself, to the metaphors and other rhetorical devices in his chosen texts. But that might have required another book, and this one, despite its shortcomings, is an engaging discussion of an important subject.

JACKSON LEARS
University of Missouri

WAYNE G. BROEHL, JR. *John Deere's Company: A History of Deere and Company and Its Times*. New York: Doubleday. 1984. Pp. xv, 870. \$24.95.

This substantial book describes the remarkable rise of Deere and Company from an encumbered blacksmith's shop in Grand Detour, Illinois, to one of *Fortune's* one hundred great industrial enterprises and, finally, to the rank of the largest farm machinery and equipment manufacturer in the world. Surprisingly, the company's chief executive officers during this industrial success story continued to be members of the Deere family. Wayne G. Broehl, Jr., was not seduced by the mythology of the steel plow; to him John Deere was one of many midwestern plowmakers, distinguished from others not by unique inventive genius but by his energy, his desire to market a good product, and his marketing skills. Apparently shaken by a narrow escape from financial disaster during the late 1850s, Deere thankfully placed the business in the hands of his youthful son, Charles, and watched benevolently while that young man retained his father's commitment to quality, broadened the firm's product lines, and developed a system of branch outlets and other successful marketing practices that placed the company solidly in the first rank of firms competing with the monster trust, International Harvester, in the early twentieth century.

Charles Deere's son-in-law William Butterworth assumed the mantle of leadership in 1907. Although more conservative than Deere, Butterworth supervised the corporate reorganization that produced the modern company, acquired additional companies, and broadened the firm's manufacturing lines to include tractors and combines. Butterworth's successor, Charles Deere Wiman, empha-

sized product development and guided the company successfully through the 1930s and World War II. In turn, Wiman's son-in-law, William Hewitt, took over and led the company in a full-scale assault on foreign markets. Although the results of this departure were mixed, the fiscal administration of the Hewitt years was sound and imaginative; the company entered the 1980s in excellent shape while the one-time colossus of the industry, International Harvester, was slipping toward disaster.

Deere and Company had its share of pacesetter machines and mechanical features, like the Gilpin riding plow of the 1870s and the tractor power-lift of the 1930s, and generations of farmers came to trust the good quality of the firm's products. But Broehl maintains that the consistent advantage of Deere and Company over its competitors lay in its marketing organization. Internally, the company was remarkably decentralized, and one of the continuing tensions throughout its history lay in differences of opinion between the advocates of plant decentralization and those who wanted greater centralized control and, in the Hewitt years, unified research and development. Broehl maintains that, with the exception of the post-World War I period, Deere and Company's relations with labor were usually cordial and its paternalism less autocratic than in many companies of the same era. The company successfully maintained the individual employee contract until World War II, but, finally, company officers saw their plants unionized. Even so, they managed to avoid the protracted strikes experienced by some of their competitors.

Broehl develops his account chronologically, using the careers of the various chief executives to provide the major chronological divisions of the story. Within these sections the approach is best described as modular, since the various chapter segments are seldom related in a close or systematic way. As the title of the book suggests, this volume is reminiscent of a standard "life and times" biography. The author is also correct in using the word "chronicle" to describe his work; he is often chatty, discursive, and anecdotal. Broehl knows his way through a corporate balance sheet, but his book is not an exercise in analytic business history. Nor does he unveil a roster of corporate villains or incompetents. We learn of the competition, but little effort is made to fit the Deere operation into overall patterns of management styles or emergent corporatism. The statistical appendixes are interesting and informative but often lack chronological sweep. Both agricultural and business historians will find much of interest in this book, however, as will some of the old Deere employees or residents of Deere company towns like Moline, Illinois, or Dubuque and Waterloo, Iowa. Despite the considerable contribution to knowledge that Broehl makes, scholars will hope

that more specialized studies of this large and interesting company will be forthcoming in the future.

ALLAN G. BOGUE
University of Wisconsin

DONALD E. OSTERBROCK. *James E. Keeler: Pioneer American Astrophysicist and the Early Development of American Astrophysics*. New York: Cambridge University Press. 1984. Pp. xii, 411. \$39.50.

On his death at the age of forty-two in 1900, James E. Keeler was recognized as the leading astronomical spectroscopist of his era. As a staff member and, later, director of the Allegheny and Lick observatories, Keeler examined the spectra of both stars and nebulae, calculating the radial velocities for many of these objects. He was also able to integrate into his studies new photographic techniques and laboratory spectroscopy, which was a crucial step forward in the creation of modern astrophysics and further solidified Keeler's position in the astronomical community. In 1898 Keeler returned to California as director of the Lick Observatory, where he spent the remainder of his life using the new Crossley reflector to study faint stars and nebulae, including those now known to be distant galaxies. Keeler's sensitive photographic plates displayed clearly the spiral structure of many of these bodies and showed the presence of large numbers of previously unknown spirals. His death in 1900 came shortly after he had begun similar studies on globular clusters. Despite his reputation at the turn of the century, however, Keeler is now largely unknown outside a small circle of historians of American astronomy. Donald E. Osterbrock has taken an important step toward correcting this oversight.

Yet this book is far more than a biography of Keeler. Because the astrophysical community was quite small, Keeler was aware of almost everyone's research. His first position involved him in Samuel P. Langley's pioneering work in solar physics, and he worked closely with the young astrophysicist George Ellery Hale on various projects, including the planning of the Yerkes Observatory and the founding and editing of *The Astrophysical Journal*. His transfer to Lick in 1898 not only provided Keeler with greater opportunities for research, but also brought him into contact with astronomers such as W. W. Campbell and Robert G. Aitken, both of whom would become significant figures in the field. Keeler's photographs encouraged Hale to build the 60-inch reflector on Mt. Wilson and led Heber Curtis to investigate in greater detail the spiral galaxies. Edwin Hubble's famous work on spirals used the Mt. Wilson 100-inch reflector and, thus, may also be viewed as an extension of Keeler's

efforts. As Osterbrock emphasizes, modern astrophysics owes much to James E. Keeler.

In his reconstruction of Keeler's career, Osterbrock mined every archival source that could be expected to shed light on the subject. He also made full use of contemporary scientific periodicals and appropriate secondary sources. Although the discussion of the technical details of Keeler's discoveries may tax the nonspecialist, such discussion serves to emphasize the increasing complexity of astronomy at the turn of the century. Osterbrock's volume is a valuable contribution to the history of American astronomy and provides important perspective on the origins of the "new astronomy" of Keeler, Hale, and others. As such, it will serve the needs of those interested in a better understanding of astronomy and those who desire a more complete view of the United States of the late nineteenth century.

GEORGE E. WEBB
Tennessee Technological University

D. STEVEN BLUM. *Walter Lippmann: Cosmopolitanism in the Century of Total War*. Ithaca, N.Y.: Cornell University Press. 1984. Pp. 205. \$19.95.

The title of this slim volume may be misleading. Free-lance writer D. Steven Blum says virtually nothing about Walter Lippmann the public figure and political analyst who, in over thirty-five years of writing the "Today and Tomorrow" column, brought a dignity and substance to American journalism that no one else has come close to matching. Blum argues that the achievements of the public Lippmann are by now well documented, and that, particularly since the publication of the biography by Ronald Steel (1980), little more needs to be said on the subject. On the other hand, he believes that insufficient recognition and respect have been paid to Lippmann's writings in political philosophy. Blum focuses entirely on that area to correct what he perceives as an imbalance in the critical literature to date.

One might easily question the premise. Is it true that such twentieth-century classics as *A Preface to Politics* (1913), *Public Opinion* (1922), *A Preface to Morals* (1929), and *Essays in the Public Philosophy* (1955) have received "comparatively little attention and few plaudits" (p. 10)? If so, this book is certainly welcome. And even if the neglect is not quite as serious as the author makes out, the book is still welcome for its many virtues. Blum writes lucidly and intelligently about the development of Lippmann's thought and about the intellectual influences on him. Moreover, although Blum admires his subject, he does not hesitate to point to weaknesses in Lippmann's thinking when he finds them. All in all, this is a fine introduction to the writings of an

individual who had much to say about the dilemmas posed by democratic values in the modern era.

Whether Blum contributes anything new is another matter. As the subtitle suggests, his central point is that "cosmopolitanism" is the pervasive and unifying theme in all of Lippmann's work. He may be right, but as he defines and applies the term the case does not seem to add up to much. By cosmopolitanism Blum means that Lippmann was sensitive to international as well as domestic issues, receptive to ideas and opinions from abroad, and, above all, aware of the interdependence of all societies in the modern world. Which is to say, he was no provincial. That is hardly a stunning insight.

It is also a nice question whether Blum is wise to draw such a sharp distinction between Lippmann the political philosopher and Lippmann the political analyst (which is, of course, what his journalism was all about). After all, the philosophical values constantly influenced the columns, just as surely as the events written about in the columns helped shape the values. The two cannot be separated without doing an injustice to both. They are parts of a whole and must be taken as such.

To say this is not meant to minimize the merits of the book. Many people will find it informative and a pleasure to read.

GEORGE JUERGENS
Indiana University

JEANETTE E. TUVE. *First Lady of the Law: Florence Ellinwood Allen*. Lanham, Md.: University Press of America. 1984. Pp. viii, 219. Cloth \$24.50, paper \$12.50.

In writing this brief biography, Jeanette E. Tuve faced the daunting challenge of Florence Ellinwood Allen's engaging autobiography, *To Do Justly*, published two decades ago. Tuve has consequently chosen her ground carefully. Rightfully asserting that Allen chose not to reveal the "inner woman," Tuve seeks not only to redress that omission but also "to trace the symbiotic relationship between the life and career of Florence Allen and the woman's movement" (p. v).

Allen's heritage and education were the traditional seedground for Progressive reformers. An ancestor on her mother's side sailed on the Mayflower; her father's family included the Allens of revolutionary heroism, whalers, and professionals. Her mother was the first matriculant at Smith College, a skilled musician, and an organizer. Her classicist father, forced by tuberculosis to settle in Utah, became that state's first congressman. Allen's education included four years at the Woman's College of Western Reserve and two years of intensive music study at Berlin, before she returned home to Ohio to teach and to become an exuberant music

critic for the Cleveland *Plain Dealer*. Secure in family and friends but restless in her teaching, she chose a career in law.

Her subsequent success was a model of women's advancement through high competence and networks forged in reform and suffrage battles. As soon as the Nineteenth Amendment was ratified, she and her women allies launched a successful nonpartisan campaign for her election to the Court of Common Pleas. This victory was followed by stunning elections in 1922 and 1928 to the Ohio Supreme Court. After an unsuccessful campaign for the Senate in 1926 and another for the Congress in 1932 on the Democratic ticket, she won appointment to the United States Sixth Circuit Court of Appeals where she wrote the landmark opinion upholding TVA. Pressing her candidacy for the next twelve openings for the Supreme Court, "women had almost a neurotic fixation in Allen as the symbol of what they would and had not achieved. She could not withdraw for she was the leader and symbol of a cause to which she had devoted most of her life" (p. 170).

Thoroughly culling the rich collections of Allen's letters and records, Tuve sensitively develops her relations with her family and women friends. The inner woman remains illusive. Allen's Junoesque outer proportions are sympathetically detailed. Taller than most, she constantly struggled with her weight through diet and rigorous exercise; with any letdown she ballooned to two hundred twenty pounds. Not blessed with a "graceful carriage," Allen "strode along like a woodsman on the way to work" (p. 21).

Structurally, the biography and autobiography are closely parallel; chapter topics are almost identical. Yet their emphasis is complementary; Allen concentrates on her work—on the bench and campaigning for suffrage and the outlawry of war. Tuve, while covering these aspects, focuses on and is surest in dealing with the life—the challenges Allen met as a single woman professional. For the fullest understanding of Allen, they should be read in tandem.

There is a sturdy quality in Allen and in Tuve's biography. Not so in its production. The sole picture is the frontispiece; the binding begins to yield after a few readings; more significant, an editor should have caught errors in typing and in substance (the statement that the Sheppard-Towner measure was "never implemented" [p. 85]) and could have ensured an adequate index. Allen and Tuve deserve better.

DOROTHY M. BROWN
Georgetown University

WILLIAM W. BREMER. *Depression Winters: New York Social Workers and the New Deal*. (American Civiliza-

tion.) Philadelphia: Temple University Press. 1984. Pp. xiv, 231. \$29.95.

When Franklin D. Roosevelt confronted unprecedented levels of unemployment, he sought to establish a welfare system that would be uniquely American. Although the United States lagged far behind the industrialized countries of Europe in shielding its citizens against economic hardship, Roosevelt's efforts to find an American approach did not begin with a blank slate. The new president's own experience in the first years of the depression as governor of the nation's most populous state provided him with a framework of policies toward relief, unemployment, and social insurance.

William W. Bremer argues convincingly in this short, interesting study that the source of much of Roosevelt's thinking on these subjects was the "social work community" of New York. This remarkable group of thirty to forty individuals may not have been as much of a "community" as Bremer thinks, but it provided two of the key figures in the New Deal—Harry Hopkins and Frances Perkins—as well as many of the less well known New Dealers. More important, though, were the ideas these New Yorkers supplied, which Bremer contends formed the basis of Roosevelt's social policy.

Most of the New York social workers came from rural, Protestant roots. They approached the problems of industrial society with the usual American assumptions about the desirability of individualism, private enterprise, the work ethic, and local responsibility. Their interest in the plight of the victims of the economic system set them apart from the majority of their countrymen in the 1920s, but in 1929 the New York social workers were still closer to the American mainstream than they were to the advanced positions many of them would adopt over the ensuing years of economic collapse.

It was the experience of attempting to use traditional means of private charity to meet the needs of New York's unemployed during the early years of the depression (particularly, as Bremer's title indicates, during the winters) that led the social workers to seek new approaches. When the depression began, most of them rejected public relief, but they were soon working closely with the Roosevelt administration in Albany to provide such governmental assistance. New York's Temporary Emergency Relief Administration (TERA) embodied most of the social workers' new principles, and the TERA became the model for the New Deal's early federal relief program.

The social workers were increasingly concerned with the psychological effects of unemployment. This led them to favor work relief over the dole, and insurance systems to which workers contributed for

security against unemployment, sickness, and old age.

The early New Deal filled the social workers with excitement. Their affection for FDR peaked with his establishment of the Civil Works Administration (CWA) in November 1933. The CWA provided real jobs at decent wages for the unemployed, without imposing demeaning poverty tests. But the president's announcement, a few weeks after the CWA began, that the agency would be terminated when spring arrived shattered the hopes of the social workers.

During 1934 the New York social workers became more disillusioned with the New Deal's halfway measures. Their influence had been substantial at the start, but the New Deal's thinking remained at the point the social workers had reached before FDR left Albany, whereas the philosophy of many of the New Yorkers continued to move leftward.

Bremer's useful book shows how the social workers' ideas gave them influence, which provided them with hope, and how that hope turned to dismay when the New Deal failed to follow their increasingly progressive proposals.

ROBERT S. MCELVAINE
Millsaps College

BONNIE FOX SCHWARTZ. *The Civil Works Administration, 1933-1934*. Princeton: Princeton University Press. 1984. Pp. xviii, 300. \$27.50.

The Civil Works Administration (CWA) is known primarily as one of the hastily improvised New Deal agencies, created specifically to get the unemployed through the winter of 1933-34 by supplementing the relief program initiated by the Federal Emergency Relief Administration (FERA). Sandwiched between the FERA and the Public Works Administration, which took up the task in 1935, the short-lived CWA is usually recalled as another expression of makeshift work relief, conceived by social workers using the means test to determine eligibility. In this book, Bonnie Fox Schwartz shatters this familiar conception by presenting a vivid, imaginative, and convincing reinterpretation of the CWA, its conceptual sources, operating principles, and its cast of professional managers.

Schwartz's thesis is that the CWA was a comprehensive departure from the FERA in public policy. Harry Hopkins, dissatisfied with the social work mentality that shaped the FERA, set Aubrey Williams at work to create a new approach that would transform relief, with its demeaning connotation of the dole, into a respected form of public employment. This meant the displacement of social workers and their methods by professionals who were distinguished by their common commitment to the

practices of scientific management and industrial efficiency. As followers of Frederick W. Taylor, the staff of the CWA included economic planners, engineers, industrial management and safety experts, statisticians, accountants, and labor mediators. Their goal was to transform a haphazard and inefficient relief experiment into a structured, cost-efficient, publicly useful enterprise that would lift the spirits of the unemployed and restore their self-esteem by having them work on genuine jobs.

One of the virtues of Schwartz's study is that she introduces a new set of characters to the drama of the New Deal. Foremost among these are Corrington Gill, a statistician who headed the Division of Finance, Research, and Statistics; Jacob Baker, in charge of engineering services; John Michael Carmody, chief engineer and labor negotiator; Sidney J. Williams, safety engineer; and Donald C. Stone, skilled in auditing and accounting. It is these men and others like them, drawn from a wide range of professional associations, who created an impressive demonstration of public employment in the CWA and provided the supportive evidence for Schwartz's thesis.

Inevitably, the innovative nature of the CWA created repercussions. In separate chapters Schwartz recounts the impact of the CWA on political bosses, the American Federation of Labor (AFL), white-collar and professional workers, and women. A basic issue here was for the managers of the CWA to maintain the organization's integrity while making suitable concessions to group demands. Schwartz shows the CWA's success in deflecting the bosses' quest for patronage, satisfying the AFL on wage scales and union hiring of skilled labor, and providing appropriate and creative jobs for white-collar workers and professionals. The organization was least successful in aiding the "forgotten" woman.

Schwartz's monograph is a valuable addition to the literature of the New Deal and will be of great interest to those exploring the evolution of scientific management in public policy from its source in the Progressive era onward.

ALBERT U. ROMASCO
New York University

D'ANN CAMPBELL. *Women at War with America: Private Lives in a Patriotic Era*. Cambridge: Harvard University Press. 1984. Pp. xiv, 304.

D'Ann Campbell's book promises a rediscovery of women's lives during World War II. Relying heavily on public opinion polls as well as more conventional sources, Campbell seeks to provide an analysis that views the war from a popular perspective and acknowledges human agency in historical processes.

Assuming the mantle of the apolitical, detached scholar, she asserts her intention "to set the record of the 1940's straight rather than to entertain or reform people today" (p. 15). Such claims to neutrality are hard to sustain in a book that reifies gender conformity in women and that is relatively uncritical of corporate management and yet implacably hostile to unions.

Although well written and well researched, this book contains major analytical flaws. Unsupported assertions and internal contradictions abound, a result of a reliance on unanalyzed and often unfounded assumptions. Campbell claims, for example, that women were alienated from unions because of the sexism of union policies, although she provides no evidence of a connection between women's attitudes toward unions and their experiences in unions. Her discussion of this issue assumes that women workers' problems were only those of gender and not those of class, that union policies were more sexist and damaging to women workers than those of corporate management, and that institutional sexism automatically prompts antagonism from women.

Campbell states that women exercise power by shaping social consensus on gender norms. This power requires no collective action or institutional basis but instead consists in the aggregate effects of individual decisions ostensibly unconstrained by social norms or economic structures. Thus, women's exclusion from authority in business, religion, the media, and politics constitutes no bar to effective social power. Unpersuasive on the surface, this conclusion is further undermined by Campbell's own evidence. She documents a pervasive sexism shared by men and women alike and sanctioned by a variety of powerful social institutions. In Campbell's analysis women's agency is reduced to a "willing" complicity in such ideas and the practices they mandated.

Many of her other major conclusions are equally unpersuasive. These include her claims that during the war years marriages became more equalitarian, prompting women to increase their commitment to domesticity and motherhood; that nurses (otherwise known as "angels of mercy") took control of their profession; that there were very few lesbians in the factories or the military; that volunteers made no real productive contributions to the war effort; and that relatively few women lost good jobs because of postwar discrimination.

Whether Campbell has established that women were "at war with" America is unclear. That her conclusions are often at war with logic, the evidence, and each other constitutes the major problem with this book.

KAREN ANDERSON
University of Arizona

MAUREEN HONEY. *Creating Rosie the Riveter: Class, Gender, and Propaganda during World War II*. Amherst: University of Massachusetts Press. 1984. Pp. x, 251. \$20.00.

World War II represents a strange interlude in the twentieth-century history of American women. For the duration of the war large numbers of women left their homes to fill, often quite comfortably, places usually occupied by men. When asked, the majority of these women responded favorably to questions concerning new jobs and greater responsibilities and expressed strong desires to hold on to them. Yet, when peace returned, few women retained their wartime positions and instead resumed domestic vocations or transferred into sex-typed occupations such as clerical and service work. Within a short time the "feminine mystique" replaced the image of Rosie the Riveter. Why did World War II bring so few long-term improvements in women's status?

Maureen Honey finds one possible explanation for this postwar reaction in wartime propaganda. She examined the prevailing images of women from the late 1930s through the postwar period and discovered, contrary to popular belief, more continuity than change. Although the war generated depictions of women in nontraditional pursuits, its chief propagandists nevertheless reaffirmed familiar values, emphasizing patriotism, of course, but mainly devotion to family. They encouraged women to work outside the home but carefully downplayed the rewards of self-esteem by underscoring the higher virtue of duty. Be they welders, nurses, or armed forces personnel, women war workers were, beneath their coveralls or uniforms, merely housewives and sweethearts helping their fighting men. Women might fill different positions, according to wartime propagandists, but they nevertheless maintained their primary feminine roles.

Although previous scholars, such as Leila Rupp, have decoded the message of this particular brand of wartime propaganda, Honey presents a uniquely detailed study of a single source, the magazine industry. Sampling from two leading publications in the fields of so-called family slicks and confessions, she analyzes and gives examples from nearly eight hundred short stories and countless advertisements featuring women. Honey also illuminates the instrumental role of government agencies—the Office of War Information, the War Advertising Council, and the Magazine Bureau—and finds a positive correlation between alternations in the magazines' depictions of women and shifts in official policy. The conventional emphasis on feminine self-abnegation derives at least partially, in Honey's view, from the government's explicit directives to the media and to their voluntary cooperation.

The most original aspect of this fascinating case study documents class differences in wartime portrayals of women. Surveying the middle-class *Saturday Evening Post*, Honey finds fictional women enjoying the rewards of, and excelling in, managerial, professional, and other high-paying war jobs or embarking on exciting adventures in military service. *True Story*, however, featured stories of women war workers in such traditionally sex-typed occupations as teaching, clerical work, and nursing. Whereas the *Post* fantasy emphasized self-actualization through the heroine's mastery of new skills, *True Story's* formula relied on promises of marriage, motherhood, and security. The magazine directed toward working-class readers, Honey concludes, departed less from conventional depictions of feminine roles, and its most positive images affirmed pride of accomplishment in class rather than gender terms.

Honey's fine study of wartime images of women underscores the importance of systematic research. In addition, her book demonstrates the value of scholarly sensitivity to issues of class as well as gender. The differences Honey finds are striking, her conclusions sobering.

MARI JO BUHLE
Brown University

MAURICIO MAZÓN. *The Zoot-Suit Riots: The Psychology of Symbolic Annihilation*. (Mexican American Monograph, number 8.) Austin: University of Texas Press. 1984. Pp. xiii, 163. \$12.95.

The Los Angeles zoot-suit riots (1943), in which military personnel attacked Mexican-Americans wearing outfits referred to as zoot suits, are familiar to historians. Although a comprehensive study has yet to be published, Mauricio Mazón does not intend to give us a definitive history. Instead, he focuses on the psychology behind the riots and their meaning as an attempt to annihilate symbolically the Chicano youths who filled the ranks of the zoot-suit gangs.

To Mazón, the episode is best understood as part of the psychic and physical upheaval caused by the war years. The perverse image of zoot-suiters as bizarre creatures, clearly identifiable by garb and argot yet elusive and of uncertain origins, first achieved public notoriety during the infamous Sleepy Lagoon trial of 1942. In April and May 1943 Sunday installments of the comic strip *Li'l Abner* gave "zoot-suiterism" wider adverse publicity, presenting millions of readers with a conspiracy by zoot-suit clothing manufacturers to take over the country. By the summer of 1943 the people of Los Angeles associated defiance and deviance with zoot-suiters and the ideal of American youth with the

thousands of uniformed servicemen stationed in the city.

If the incompatible images of military men as patriotic Americans and zoot-suiters as un-American delinquents made a clash between the two inevitable, so too did the atmosphere of wartime Los Angeles foster friction. By 1943 civilians complained about the imposition of military personnel on their city; in turn, the servicemen seemed inclined to release accumulated psychological tension by "letting off steam." Frequent skirmishes between servicemen and civilians occurred, but newspapers gave greater space to the more isolated clashes between servicemen and zoot-suiters.

In the chapter entitled "The Symbols, Images, and the Rhetoric of the Riots," Mazón skillfully brings the riots into focus by describing them as a symbolic style of mass protest. When the riots broke out in June, servicemen and civilians united and acted out repressed fears about the war in a complex series of symbolic rituals of death, rebirth, initiation, and role reversal. The public clamored for the physical annihilation of the zoot-suiters, and the behavior of the servicemen took on a semblance of patriotism against the surrogate enemy. The servicemen restrained themselves to symbolic ceremonies in which they cornered zoot-suiters, gave them military haircuts, and then proceeded to "unpant" them. Such actions, the author observes, were a reaction to the infantilization and symbolic castration of basic training: the soldiers did to Mexicans what had been done to them. In periods of high stress, Mazón tells us, subordinated people identify with the source of power; the recruits thus assumed the role of drill instructors and zoot-suiters were assigned the part of new recruits. Through identification with the aggressor, role reversal, and projection and displacement of their anxieties, servicemen alleviated their feelings of depersonalization by "letting off steam."

In the nascent field of Chicano history psychohistorical studies are not abundant. Thus, Mazón makes an immense contribution to the study of the Mexican-American. He shows the exciting possibilities of combining two disciplines and presents evidence of the growing sophistication of Chicano history. We now know that complicated deep-seated psychological motivations are responsible for acts of violence throughout American history, including those directed against Chicanos. Our duty is to follow this exemplary study by examining similar episodes in Chicano history with Mazón's insight and erudition.

ARNOLDO DE LEÓN
Angelo State University

THOMAS M. LEONARD. *The United States and Central America, 1944-1949: Perceptions of Political Dynamics.*

University: University of Alabama Press. 1984. Pp. xii, 215. \$21.75.

The prospective reader who looks in Thomas M. Leonard's book for a background analysis revealing the origins of Washington's contemporary problems in Central America will be disappointed. To be sure, the mid-1940s were crucial in the modern history of the region, for at that time several tenacious dictators were overthrown without any noticeable improvement in the quality of government, and the United States followed events only casually, turning its principal attention away from Latin America to Europe and the Far East. Leonard's title, however, is misleading, for, excepting a brief introduction and a few useful concluding remarks, he has little to say about American policy decisions. The subtitle indicates the real purpose of the book—to show how American diplomats perceived Central American political dynamics in their day-by-day reports to the State Department. In effect, the result is a detailed narrative of each country's governmental crises, alliances, and intrigues.

By "political dynamics" Leonard means the rivalry of local oligarchies, the pressure of the middle sector for constitutionalism, splits within the military, and the increased role of communism, especially among the lower classes. His sources are almost entirely diplomatic correspondence, with occasional references to the *New York Times* and a few monographs. Since few embassy writers stayed in one post long enough to gain a "feel" for the country and since Washington apparently paid little attention to their reports anyway, one gets the impression of unreality, of masks in a pageant, remote from the really important affairs of the world. Thus, although the embassy personnel in El Salvador agreed that the country was ripe for social revolution, the embassy secretary was sure that it was too unimportant to attract either Soviet attention or aid and that, since the average citizen was apolitical, he would probably not develop allegiance to any political ideology.

At the end of the period covered by the book Fidel Castro was still more than a decade away from power, so perhaps the embassy secretary's myopia was excusable. Still, a historical account of recent Central American developments would benefit from more sense of imminent disaster. Leonard does not even give his reader much preparation for the notorious American intervention of 1954 in Guatemala, for, although he discusses Jacobo Arbenz (along with a host of other politicians), he virtually ignores the activities of United Fruit in Guatemala and, indeed, throughout the region. His account of Nicaragua would be improved by more background on earlier American interventions, especially during the late 1920s. All in all, the chief values of the book are its detailed information about

Central American political complications through half a decade and a concise summary and appraisal of these events at the end.

DAVID M. PLETCHER
Indiana University

TONY FREYER. *The Little Rock Crisis: A Constitutional Interpretation*. (Contributions in Legal Studies, number 30.) Westport, Conn.: Greenwood. 1984. Pp. xii, 186. \$27.95.

This book by Tony Freyer is an in-depth analysis of the public school desegregation crisis that occurred in Little Rock, Arkansas, in 1957-59 in the wake of the U.S. Supreme Court's decision in *Brown v. Board of Education* (1954) that racial segregation in the public schools was unconstitutional. The events in Little Rock not only came to symbolize southern defiance of federal authority on the issue of racial desegregation but also revealed the difficult and complex problems that beset attempts at fundamental societal change through the exercise of judicial power. Indeed, viewed from a contemporary perspective, the Little Rock crisis serves as a profound reminder not only of how far we have progressed but also of how much remains undone as we grapple with America's most difficult domestic problem.

As Freyer correctly notes, that a major crisis over racial desegregation in the public schools occurred in Little Rock is ironic since, by the 1950s, that city was widely regarded as progressive on issues of race and because moderates dominated the politics of the community. Orval Faubus also seemed at the outset an unlikely candidate for becoming a symbol of defiant resistance to desegregation, because he was viewed as a progressive, even liberal, figure in the politics of Arkansas. Using previously untapped sources, including FBI files, NAACP papers, and private legal files, Freyer deftly analyzes the forces that transformed both Faubus and Little Rock from symbols of moderation into symbols of disobedience on the desegregation issue.

Freyer demonstrates that Faubus's concern for his legislative program as well as his political ambitions led him to respond increasingly to political pressure generated by segregationists in Arkansas. The Eisenhower administration's timidity in enforcing desegregation, however, made it difficult for political leaders in Arkansas, and throughout the South generally, to divert the focus of segregationist political pressure to the national government and away from state and local leaders such as the Little Rock school board and Governor Faubus. In addition, Freyer notes, desegregation in Little Rock had been presented to the public as a legalistic necessity to comply with federal law, a rationale that could be undermined if alternative legal theories were pre-

sented by segregationists denying the constitutional legitimacy of desegregation. The segregationists in Arkansas presented just such a theory, the doctrine of interposition, that confused the public about the legitimacy of desegregation and offered hope that the painful prospect of desegregation could be avoided altogether.

The Little Rock crisis, Freyer concludes, was therefore a product of a complex interplay among national, state, and local political factors that interacted with the law and the judicial process. He has skillfully dissected the crisis in his analysis and, in the process, has made a fine contribution to our understanding of the interaction of law and politics in American society.

RICHARD C. CORTNER
University of Arizona

BRUCE PALMER, JR. *The 25-Year War: America's Military Role in Vietnam*. Lexington: University Press of Kentucky. 1984. Pp. ix, 236. \$24.00.

In this short, fact-filled, and thoughtful book, General Bruce Palmer, Jr., a participant in the Vietnam War, analyzes reasons for the American failure in that lengthy, costly, and divisive conflict. In his account he manages to keep a balance that will prove helpful to the scholar, the soldier, and the man in the street. The son of a World War II general, Palmer graduated fifth in the class of 1936 at West Point, well above his better-known classmates Generals William Westmoreland and Creighton Abrams, under whom he served in Vietnam and in the Pentagon. His experiences in World War II, Korea, and Vietnam give him a valuable perspective for his study.

Palmer maintains that the basic mistake of Presidents Kennedy, Johnson, and Nixon was the failure to make clear what the country's aims were in Vietnam. This led to the failure to get the all-out support needed to win. The presidents tended to believe not only that it was possible to win the war by gradually increasing the supply of war materiel but also that it was possible to get effective popular support even without warning the public of the cost. In addition, to avoid unpopularity the presidents made no effort to pay a proper share of the expenses as the war progressed. For a considerable period the management of the draft system put a disproportionate share of ground losses on underprivileged groups. This contributed to a lowering of morale, which showed itself in the "fragging" of junior officers and increased drug use. Finally, the rotation system tended to weaken the cohesion and battle experience of troops.

Having observed at first hand the relations of civilian leaders with commanders in the field and of

the White House with the Pentagon, Palmer is caustic about the tendency of White House advisers to bypass service secretaries. He points to problems created by Henry Kissinger's proclivities in this connection, especially noting the liberties that Kissinger permitted his assistant, General Alexander Haig. Palmer notes, for example, that, when Haig succeeded him as vice chief of staff in the Pentagon, he installed a special telephone system that allowed him to communicate directly with Kissinger or the president without the knowledge of civilian or military chiefs in the Pentagon. Palmer also maintains that the decision to put overall activities of military forces in the Pacific under the commander in chief, Pacific, led to control too distant from the fighting area. He believes that the failure to bring South Vietnamese troops more directly under U.S. operational command frequently led to lack of effective coordination. In particular, he argues that control of intelligence was too divided.

Although restrained in his criticism of the commanders in chief of the period, he faults Johnson for fighting the war primarily for political benefit. Nixon, he writes, became obsessed with winning, particularly after the Watergate revelations weakened his position. Frantically struggling to get an armistice, Nixon hurt his case by letting the enemy sense what he was up to. Although Palmer believes that substantial grounds for presidential action in connection with Laos and Cambodia existed, he suggests that the secrecy with which these attacks were handled weakened the confidence of the public in the White House without aiding the war effort.

FORREST C. POGUE
Washington, D.C.

ALEXANDER P. LAMIS. *The Two-Party South*. New York: Oxford University Press. 1984. Pp. x, 317. \$25.00.

Nearly four decades after the publication of *Southern Politics in State and Nation*, V. O. Key still towers over more recent scholars working in the field. No book on southern politics is complete without appropriate quotations from the master, and his format of state-by-state analysis is routinely emulated by those seeking to update his work. Very much a part of this tradition, Alexander P. Lamis also responds to the work of Key's associate, Alexander Heard, by modifying Heard's 1952 title, *A Two-Party South?*, into *The Two-Party South*. The focus of Lamis's work is on what he calls the "post-civil rights era," the period when racial issues began to wane and southern politics increasingly responded to the "New Deal-style partisan alignment" characteristic of the rest of the nation. In the early 1960s the dispute over civil rights nurtured the growth of southern Republicanism, Lamis finds, and the party

gradually became an acceptable home for social and economic conservatives. But Lamis argues that Republicans are not really captains of their fate in Dixie because Democrats remain the majority party—when they can assemble an alliance of blacks and lower-class whites. With some exceptions, the Republicans are usually successful only when the Democrats fail at coalition building.

Writing in an engaging manner that blends colorful quotations and quantitative analysis, Lamis offers a concise portrait of each state, but particularly appealing is his final chapter that draws on survey research to provide some generalizations about the current status of southern politics. He concludes that the New Deal-style alignment now appears in the political system from the national level to the local level, but this development is significantly more advanced in the Rim South than it is in the Deep South. He cites data indicating that black and white Democrats are moving closer together and that the center of the party is moving leftward. The emergence of liberal leaders who are responsive to this trend will drive the conservatives into the Republican party, leaving the South with a liberal-conservative party alignment similar to that of the nation as a whole.

Lamis's work has implications for two hearty perennials in the field of southern studies. First, although he does not address this issue directly, Lamis apparently believes that the South still has a regional distinctiveness. He suggests Florida and Texas are no longer truly southern but sees the other nine ex-Confederate states as part of a "persisting political South" linked by the struggle to build the black-white coalition. Moreover, like Key and others before him, Lamis is interested in how the current system effects "those who have less" in southern society. A liberal Texan, Key believed the South would fervently embrace a New Deal-Fair Deal program once the props of the conservative system were destroyed. Has a political structure responsive to the needs of the "have-nots" evolved in the post-civil rights era? "Not yet," Lamis says, "but it could be coming" (p. 232). Apparently, the old Populist dream of a more liberal South is as fascinating and as elusive for southern scholars as it is for southern politicians.

DAVID D. LEE
Western Kentucky University

RICHARD L. SCHOTT and DAGMAR S. HAMILTON. *People, Positions, and Power: The Political Appointments of Lyndon Johnson*. (Administrative History of the Johnson Presidency.) Chicago: University of Chicago Press. 1983. Pp. x, 245. \$18.00.

Of all the marks that a president leaves on his segment of history, none is so telltale as the appoint-

ments he makes. Especially in the present age, when only about thirteen hundred positions are at the chief executive's disposal, his assignment of them illuminates the character as well as the imaginativeness of his official conduct. The study of presidential patronage for the forty presidents has been only fragmentary. It is certain that scholars hereafter taking up this fertile subject will find that in this work on Johnson's appointments Richard L. Schott and Dagmar S. Hamilton have supplied a worthy template for comparable monographs.

Lyndon Johnson operated on two tracks in filling the jobs required to man—and to “woman”—his administration. One was to seek out the best possible candidate for each place after a painstaking search conducted under the direction of John Macy, chairman of the United States Civil Service Commission, who acted as Johnson's appointments adviser. (I can testify to the thoroughness with which even minor posts were filled under the “Macy operation,” having received telephone calls from the White House several times in the late 1960s for suggestions of names to serve on Fulbright award committees—suggestions I knew would be compared with those provided by other professors.)

With Macy's devoted help, Johnson, in his first year in the White House, put his stamp on the presidency he had inherited. By July 1965 he could tally 265 major appointments. But Macy's influence waned as the war in Vietnam, then heating up, took more and more of the president's energy and gradually sapped Johnson's enthusiasm to remake the executive branch in his own image. Macy, moreover, could observe that the intractable war imposed on the president “an increasing paranoia about certain groups in the society and therefore an increased limitation on the range of the search” for which he called (p. 30). As the administration wound down, few important searches were conducted in making even major nominations. Johnson's second method—a reliance on personal connections and word-of-mouth suggestions—became his chief *modus operandi*. As his beleaguering increased, he apparently found personal security in accepting the suggestions and advice of proven friends inside and outside of government. Whatever the yardstick may be for judging the merit of appointees, Johnson's post-1966 choices—like the later ones of all the modern presidents—were mostly marriages of convenience, not infusions of fresh strength carefully ferreted out.

The authors are at their best in recounting how Johnson decided the qualification for office of some of his cabinet designations. For example, his tie to Alexander Trowbridge, the able businessman who served briefly as secretary of commerce, seems to have been annealed by the heart trouble that afflicted both men. Using the oral histories in the LBJ

Library and wide-ranging interviews, Schott and Hamilton show Johnson often twisting arms and teasing people into accepting office. The appointment of Clark Clifford as secretary of defense at a watershed moment is a stunning example of how a historic role is sometimes played despite the player, for it is revealed here that Clifford had earlier been asked to be undersecretary of state and twice had been offered the attorney generalship.

Schott and Hamilton are justly able to conclude that the Johnson appointees on the whole were “persons of substantial character and high ethical standards who led an administration remarkably free of the scandal, personal pettiness, and adventurism which, unfortunately, came to characterize the presidency that was to follow” (p. 209). How favorably an administration is remembered depends, though, not only on high-minded officials and effective personnel procurement but also on good luck. Alas, no system, formal or informal, has been devised to yield this ingredient.

HENRY F. GRAFF
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CARL SOLBERG. *Hubert Humphrey: A Biography*. New York: W. W. Norton or Stoddart, Don Mills, Ontario. 1984. Pp. 572. \$19.95.

Carl Solberg has written the first scholarly biography of Hubert Humphrey. Until now, accounts of the “prairie Progressive's” career have been monopolized by muckrakers and friends. Solberg's study supersedes them all by supplying a much more solid evaluation, one that does for Humphrey what most political biographies fail to do for their subjects.

Solberg's thorough and sensitive portrait is a perfect example of the maxim that a good biographer must empathize with his subject. That Solberg does. But he also weeps, for, alas, the all-too-human Humphrey was deeply flawed and craved to be liked. In the book's most poignant, most revealing, and, potentially, most controversial section, he is shown to be a pathetic victim of the fight for political survival during the Johnson presidency.

The account of Humphrey's imprisonment as LBJ's vice-president receives extended treatment, thereby undercutting the importance of his service in the Senate. Although chronologically disproportionate to the Minnesotan's long public career, an understanding of his years as vice-president are essential for insights into both Humphrey and President Johnson.

Solberg is firm about many aspects of that relationship. Humphrey's dissent from the escalation of the Vietnam War was both prompt and vigorous. The vice-presidential rebellion led to swift and long-lasting “hazing” from the White House. Together

with his wife, Humphrey made a conscious decision to preserve his political future by embarking on "a new era for them." As Solberg explains, "In all public statements from that day, Humphrey had loyally upheld the President's war policy" (p. 345). The skeptic thus became the administration's leading spokesman, a "hatchet man" (pp. 285-300) for Johnson's Vietnam policies.

After Johnson's withdrawal from the 1968 campaign, the White House's interest in the Humphrey candidacy was most passionately aimed at using the vice-president to stop Bobby Kennedy. The assassination of the second Kennedy, therefore, virtually scuttled Humphrey's chances of election. Businessmen fearful of Bobby's nomination then felt free to contribute to the Republicans. Nor did Johnson any longer need to modify his war policy to keep his ex-attorney general from prevailing at the convention. Kennedy's death "disclosed damaging evidence of his [Humphrey's] predicament as vice president in a beleaguered administration—and of the personal qualities that contributed to his 'entrapment' in that position" (p. 340). At the Chicago convention in 1968, Johnson's operatives took charge, overturning a Humphrey attempt to include a platform plank conciliatory to the doves. Humphrey, already suffocated by the president, also became Mayor Daley's prisoner. Opposing Nixon, he was cut off from the \$600,000 of the President's Club, Johnson's reservoir of largess from business contributors. Humphrey's defeat that November, Solberg concludes, came mainly from "the inability of Progressivism to command more than a fleeting majority in the dynamic of the conservative country's politics" (p. 408).

One of the many virtues of this biography is that the reader will also have no problem tracing Humphrey's troubles to the same psyche that was ashamed of having engineered the Communist Control Act of 1954 but had allowed ambition to prevail. No serious examination of this crucial era can afford to ignore Solberg's superb biography.

HERBERT S. PARMET
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MICAELA DI LEONARDO. *The Varieties of Ethnic Experience: Kinship, Class, and Gender among California Italian-Americans*. (Anthropology of Contemporary Issues.) Ithaca: Cornell University Press. 1984. Pp. 262. Cloth \$29.95, paper \$9.95.

As a scholarly endeavor, this book stands as a successful exercise in futility and obfuscation. But as a primary source the book should have great value for social scientists interested in the painful search for ethnic identity by an educated Italian-American woman. Trained as an anthropologist, Micaela Di

Leonardo found the journey toward the roots of her ethnic identity more enticing than the study of some exotic culture. She was neither impressed nor happy with the existing American literature that characterizes Italian-Americans as "impoverished or working class, immersed in family concerns and uncaring for the general welfare, present oriented, unable to cooperate, fatalistic and patriarchal" (p. 20). Specifically, she blamed social scientists for studying ethnicity apart from "the larger economy and its evolutions, and from its entire ethnic/racial infrastructures of institutional racism; ethnic/racial migration; job capital, housing, and educational networks; societal ethnic/racial economic stereotypes; and union seniority systems" (p. 21). Moreover, she found their lack of interest in gender identity totally unacceptable.

With the ardor of a neophyte, Di Leonardo tackled the modest project of redefining the concept of ethnicity while establishing "new links among the histories and contemporary realities of class, kinship, culture, and gender in America" (p. 236). To achieve this end, she rejected the two commonly used research designs of random sampling and community studies. The former was found wanting because it overlooks "the casual and intimate relationships created by many social visits over time"; the latter was too narrow to study the varieties of Italian-American ethnic experiences, since "communities tend to be class-homogeneous" (p. 26). The alternate design is long and unstructured interviews with members of eighteen Italian-American families living in the San Francisco Bay area. According to Di Leonardo, the eighteen families offer a "spectrum of families representing a broad cross-section of the general Italian-American population in both a historical and contemporary sense" (p. 32). The author is not unaware that eighteen families constitute a rather small sample for such a study, but, she argues, "I have sacrificed numbers of informants for data quality and depth" (p. 32).

This reader had major problems with the depth of the author's design and arguments. The first problem is her writing. If some degree of professional jargon is tolerable in a scientific book, the writing should never be so vague and confused as to leave an educated reader uncertain about the meaning of a sentence or paragraph. The second problem is some general assumptions that even an undergraduate student might find shocking. For instance, according to Di Leonardo, "Italian immigrants and their antecedents were already accustomed to living within a national economy integrated into the world capitalistic system" (pp. 63-64). To posit the existence in Italy of a national economy integrated into a world economy at the time of mass emigration to the United States reveals the author's unfamiliarity with economic history.

The third problem is the size of the sample. Random sampling and community studies are time-tested tools devised by social scientists to reduce the risk of personal bias. To attempt to prove the limitations of these two research tools through the use of unstructured interviews with members of eighteen families is surely naive. The fourth problem is the use the author makes of her sample. Armed with these eighteen sets of interviews, Di Leonardo feels qualified to redefine a whole array of basic social concepts: "family, mobility models, kinship realities, ethnic ideology, economy, personal and collective ethnic identities, women, work and ethnicity."

The futility and obfuscation of this book as a scholarly product should not prompt the conclusion that the study is useless. On the contrary, the reading of this book as a primary source is a must for students of ethnicity. Both the style and the content convey the struggle of an ethnic American caught between the nostalgia for a comfortable and unquestioned ethnic identity and the effort to establish a link between the identity and the larger American culture. Di Leonardo is at her best when she describes long conversations over wine and plenty of food, in beautiful California surroundings, at sunset, with rather successful Italian-Americans, who feel comfortable with the material culture of their relatively new country. As an Italian-American with a higher education, however, Di Leonardo is somehow out of place in those surroundings. She obviously cherishes her status as an educated woman, but the people she interviews make her constantly aware that her willingness to analyze ethnicity sets her apart from them. Probably, Di Leonardo's ambivalence reflects the ambivalence of many Italian-Americans: if you analyze your experience, that experience is likely to lose its genuineness. The choice is painful. Italian-Americans might not be allowed to avoid it for much longer.

DINO CINEL
Newcomb College

THOMAS D. BOSWELL and JAMES R. CURTIS. *The Cuban-American Experience: Culture, Images, and Perspectives*. Totowa, N.J.: Rowman and Allanheld. 1984. Pp. xiii, 200. \$36.50.

Since Fidel Castro's ascent to power in January 1959, the number of people of Cuban descent in the United States has leaped from forty thousand to one million, roughly equal to one-tenth of the current population of Cuba itself. Now the third largest element in the U.S. Hispanic mosaic, Cuban-Americans numerically trail only Mexican-Americans and Puerto Ricans. For a brief factual introduction to this increasingly significant element

of U.S. society, this informative but uneven book would be a good place to start.

Revealing in its sociological aspects, fair but spare in its historical sections, and disappointingly superficial in its discussions of Cuban-American culture, the book reflects a division of scholarly labor. Thomas D. Boswell wrote the historical chapters on Cuba and Cuban immigration, as well as those on Cuban settlement patterns, politics and ideology, family and youth, and the demographic profile of Cuban-Americans. James R. Curtis wrote the chapters on Miami, language and religion, artistic expression, and cuisine and folkways, as well as the introductory chapter on Cubans in contemporary American society.

This fact-jammed book succeeds best as a socio-cultural synthesis of the current Cuban-American situation. Statistics often document the expected—that second-generation Cuban-Americans are younger and speak more English than first-generation ones—but sometimes reveal surprising societal changes. For example, rather than spreading throughout the United States as have other Hispanic groups, Cuban-Americans have recently been involved in a massive return migration from other parts of the country to Florida. Those who live in Florida leaped from 46 percent of all Cuban-Americans in 1970 to nearly 60 percent in 1980, and the later figures do not even reflect the additional one hundred twenty-five thousand Cuban refugees who arrived shortly after the census. In addition, Cuban-Americans are becoming increasingly suburban (now 60 percent). Moreover, nearly half of all second-generation Cuban-American women marry non-Hispanics (a much higher percentage than among other Hispanic women), thereby creating a threat to the future of the traditional Cuban-American family. In the nonquantitative realm certain descriptive sections stand out, such as those concerning Little Havana and the Cuban-American "municipalities in exile," formal self-help organizations composed of people from individual towns in Cuba.

While the book satisfies in some respects, it disappoints in others. The discussions of culture and expression are flat, lacking examples of prose and poetry and providing instead encyclopedia-style listings of specific works and minibiographies of artists and writers. For example, Ivan Acosta's unique and significant Spanish-language film, *El Super*, is treated summarily in eleven words. There are inexplicable omissions, such as the absence of any discussion of the complex role of Cuban-Americans in the elections of Maurice Ferré, the Puerto Rican mayor of Miami. The book is also marred technically by poorly reproduced photographs and gray reproductions of Cuban art. Finally, the absence of

a bibliography weakens the book's value as a reference tool.

Yet despite these flaws this is a solid, useful compendium, a good initial source for those interested in basic information about Cuban-Americans.

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Riverside

LATIN AMERICA

DIANA BALMORI *et al.* *Notable Family Networks in Latin America*. Chicago: University of Chicago Press. 1984. Pp. vii, 290. \$27.00.

This book marks the coming of age of family history in Latin America. Building on a decade of publications on elite families as well as their own case studies, Diana Balmori, Stuart Voss, and Miles Wortman present a bold synthesis of Latin American history from 1750 to 1880 through the lens of three generations of "notable family networks," or associations of elite families interconnected by marriage. A rare work of collaboration among scholars who study different countries, it shows how much comparative research can add to our understanding of Latin America.

The first chapter argues that a series of family networks formed in the late eighteenth century and came to dominate regional or (in small nations) national affairs in the nineteenth century. It sketches how family members manipulated kinship to achieve and maintain power, and it traces shifting patterns of occupation, property-holding, residence, marriage, and relations to the state. The next three chapters illustrate regional variations in Guatemala, northwestern Mexico, and Buenos Aires. The conclusion reviews the literature on Latin American family history to demonstrate how it supports these general patterns.

One may quibble with certain points, such as the authors' assertion that family networks were less important among the lower classes, a conclusion based on the mistaken notion that, because poor people resided in nuclear family households, they did not have extended family networks. These networks have been less visible to historians, but, as demonstrated by Larissa Lomnitz for Mexico and by Susan Bourque and Kay Warren for Peru, kin networks could make or break people at all levels of society. One may also question how well their chronology, derived from three marginal areas of the Spanish empire, applies to other areas where there may have been considerable continuity between the old colonial families and the "new" families founded by eighteenth-century immigrants. If the authors

had traced descent through the female as well as the male line, their concept of a "first" generation might have proved unworkable.

Still, the authors' view of the nineteenth century as the golden age of elite family networks makes sense, since the breakdown of colonial governmental and financial structures allowed families to expand their power in the public realm. Their view of families as agents of social change also makes sense, since they show that the political divisions and *caudillismo* of the nineteenth century cannot be understood without focusing on family groups, as well as social classes and regions.

Like all good books, this one raises as many questions as it answers. The description of marriage patterns implies that there was more social mobility in the late eighteenth century than in the late nineteenth, a hypothesis that needs to be tested. The description of how families behaved on the outside needs to be complemented with an analysis of their inner workings. Was family strategy consciously planned, or was it the result of autonomous choices by individuals who identified their own interests with those of their family? If strategy was planned, were decisions imposed by a patriarch or arrived at in common? Did the power of women in notable families decline in the twentieth century when, as the book indicates, the locus of association moved from the family to male-centered professional organizations, clubs, and political parties, when the state gained in strength, and when corporations increasingly replaced family businesses? Future studies addressing such questions for both the colonial and national periods will find in this book a theoretical framework that must be taken into account.

SILVIA M. ARROM
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STEPHEN ALEXANDER FORTUNE. *Merchants and Jews: The Struggle for British West Indian Commerce, 1650-1750*. (Latin American Monographs, second series, number 26.) Gainesville: University Presses of Florida, for the Center for Latin American Studies, Gainesville. 1984. Pp. xiii, 244. \$18.00.

Scholars have long known that Jews were among the merchants who played an important part in the economic and social development of the early European colonies in the Caribbean. This volume by Stephen Alexander Fortune is, however, the first detailed study of Jewish settlement and mercantile activities in England's two principal sugar colonies, Barbados and Jamaica.

Jews arrived with general population growth and the development of sugar cultivation on "the new West Indian frontier." Most Jews in those islands had Portuguese surnames. Their numbers are dif-

difficult to establish, but numbers mattered less than function. The wealthier Jews were wholesalers, primarily middlemen who operated independently of monopoly companies. Through loose but well-established commercial and financial networks in Europe and North America, they facilitated the flow of bullion, cash, credit, and those secondary staples, dry goods. They served as factors for Christians and Jews in London and elsewhere in Europe and the Americas. Like most contemporary merchants, they traded both legally and illegally. Much of their commerce was in reexported textiles and went to Spanish America in return for specie. Some were involved in the slave trade; a few owned plantations. Less wealthy Jews tended to be retail shopkeepers, thus performing a vital role in the internal economies of the two colonies. Although their Christian competitors complained bitterly about them and demanded their expulsion, both metropolitan and local governments favored their presence as generators of commerce and state revenue. Yet Jews could not hold public office, and they paid for certificates of residence and up to four times as much in taxes as white Protestants. Keeping largely to themselves, Jews tended to split along lines of European origin and degree of rigor in religious observance.

This volume not only sheds light on the specific activities of merchants and Jews in Barbados and Jamaica but also helps to enlarge our understanding of the complexity of the formation of Atlantic capital. Fortune makes two contributions in particular. First, by asserting that from the later seventeenth century strong reciprocal relations between plantation staples and metropolitan industrial needs advanced economies on both sides of the Atlantic, he argues an early date for "the foundation stages of the Industrial Revolution" and strengthens the claim that long-distance commerce stoked English manufacturing. Second, by adding still more to what we are learning about the importance of independent traders and contraband trade in the emerging Atlantic world, Fortune raises important questions about the appropriateness of Fernand Braudel's emphasis on monopoly companies and legitimate exchange.

PEGGY K. LISS
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RON SECKINGER. *The Brazilian Monarchy and the South American Republics, 1822–1831: Diplomacy and State Building*. Baton Rouge: Louisiana State University Press. 1984. Pp. xvi, 187. \$20.00.

In this slender volume Ron Seckinger argues that the newly independent nations of South America consciously used "balance-of-power diplomacy" in

their relations with their neighbors during the first decade after independence. "South American leaders revealed their familiarity with prevailing European concepts of competition for relative advantage and of equilibrium maintenance" (p. xiii). Brazil was the largest nation and only monarchy on the continent, and, as heir to the Portuguese cause in the postcolonial rivalry with Spanish successor states, it played a pivotal role in the maneuvering of power politics. Border conflicts, trade rivalries, fears of domination by ambitious neighbors, and idealistic efforts to develop international cooperation characterized the diplomacy of the period.

By the early 1830s diplomatic contacts and activities were in steep decline. Simón Bolívar was dead and with him the ideals of the Panama Congress (1826–28); Juan Manuel Rosas was turning Argentina into a dictatorship; and Dom Pedro I abandoned his Brazilian throne, paving the way for a decade of weakness. Disorganized and weak, the South American nations could not maintain the levels of diplomatic activity of the 1820s.

In organizing and analyzing the intra-South American diplomacy of the 1820s, Seckinger has made effective use of the archival resources in South America as well as of the reports of British diplomats and other observers. He has largely ignored the impact of European interests and diplomacy to focus on South American issues in an arena where no nation had an overwhelming advantage. Although the book is generally well organized, emphases are occasionally misplaced; for example, virtually the same space is devoted to a brief Brazilian incursion into Bolivia as to a three-year Argentine-Brazilian war for control of the Banda Oriental del Uruguay.

Seckinger's conclusion that the South American nations were withdrawing into isolation by 1830 reveals the most serious flaw of his study. Two decades ago Robert N. Burr propounded the thesis that Chile developed "balance-of-power diplomacy" to buttress its power in South America. This approach had its origin in the successful war against a Peruvian-Bolivian Confederation (1837–40). It developed into a regional system and achieved continental proportions following the War of the Pacific (1879–83). Seckinger's effort to apply Burr's thesis to the 1820s and his conclusion that the decade "established the main characteristics of South American diplomacy" (p. 164) obscure the colonial roots of the era. The bickering and diplomacy of the period reflect the efforts of the generation of the "liberators" to preserve intact after independence the American empires built by Spain and Portugal. These efforts failed in Spanish America and almost failed in Brazil. By 1831 those who forged independence were gone; their successors were left to transform the shards of empire into viable nations. Only

when this process was well advanced could regional and continental diplomacy emerge.

GEORGE P. BROWNE
Seton Hall University

THOMAS J. SPINNER, JR. *A Political and Social History of Guyana, 1945–1983*. (Westview Replica Edition.) Boulder, Colo.: Westview. 1984. Pp. xv, 244. \$23.50.

It is hard to study Guyana's modern political history without developing a deep sympathy for Cheddi Jagan, embattled Marxist-Leninist, East Indian, dentist-turned-politician. In this clearly written history Thomas J. Spinner recounts the rise and fall of Jagan and the subsequent rule of his erstwhile comrade, Forbes Burnham.

Early on one gets an excellent sense of the heady atmosphere of postwar Guyana, when fortune seemed to run ahead of the wildest imaginings of Jagan, Burnham, and their fellow labor and political organizers. Britain's suspension of the first Jagan government in 1953 is correctly seen by Spinner as wholly unjustifiable. And the chain of events this action precipitated in the struggle for control of the multiracial People's Progressive party (PPP) was deeply tragic for polity and society alike, culminating in a wave of CIA-abetted strikes and riots (1962–63) and the final ouster of Jagan (1965).

What followed was political cynicism and abusive manipulation of power on a massive scale. But Spinner is much less interested in seeking understanding of the Burnham phenomenon than in arousing judgment. The more he achieves the latter, the more baffled the reader becomes about the social fabric and dynamics of this immobilized country.

Spinner has us imagining Guyana as exhibiting both racial polarization and class conflict (with a multiracial mass opposed to the government), but he does not produce evidence to clarify the contradiction or to demonstrate either phenomenon effectively. He does, however, report large-scale out-migration and reviews the convincing evidence of fraud in each of Guyana's last three national elections. Burnham's base of support, he writes, "slowly eroded until it rested on an African tripod, composed of highly paid government bureaucrats, the police, and the army" (p. 140). The radical embrace of most of Jagan's Marxist program of nationalizations—in bauxite, sugar, banking, and commerce—provided Burnham more spoils for the black bourgeoisie but did not lead to a socialist redistribution of benefits. If possible, however, one would like to see demonstrations of these allegations.

Spinner notes that the term "Marxist-Leninist," in its appropriation by Burnham's People's National Congress (PNC) as well as by the newer Working

People's Alliance (WPA), "has been so abused it is almost meaningless" (p. 210). Yet this is precisely where some analytic distinctions by the author would have been welcome. Indeed, the curious inability of Jagan to make common cause with the multiracial WPA is one of the critical puzzles of Guyana's politics today. Spinner sketches out the dimensions of Guyana's problems with accuracy and clarity. But one misses a deeper probing through interviews or systematic field research.

Stylistically, the book is also marred. Especially in the last chapter, an overly rigid chronological approach and the use of just a few sources give the text a mechanical cadence. Although subheadings would have been useful throughout the book to separate the important stories Spinner has to tell, they would have come at intervals of one per paragraph in the last chapter, because the author speeds through his data with little regard for smooth textual development. But this said, there is no more complete rendering of Guyana's recent history available today.

EDWARD M. DEW
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NATALIO R. BOTANA. *La tradición republicana: Alberdi, Sarmiento y las ideas políticas de su tiempo*. (Colección Historia y Sociedad.) Buenos Aires: Sudamericana. 1984. Pp. 493.

Sponsored by the Instituto Turcuato di Tella, this latest work by Natalio R. Botana is an important contribution to Argentine intellectual history. Botana is concerned with the effort to create a republican and democratic tradition in Argentina after independence from Spain in 1810. He approaches his subject through the writings of Domingo F. Sarmiento and Juan Bautista Alberdi. Both men, would-be authors of a significant political theory, were "spontaneous creators of political thought" (p. 12), and their influence, overwhelming in their day, is still felt.

The monograph is divided into two parts. Part 1, consisting of 259 pages, is devoted to re-creating the intellectual world that existed between the mid-eighteenth century and the late 1880s, with particular attention given to the thoughts of Montesquieu, Rousseau, and Tocqueville. In part 2 Botana examines closely the ideas of Alberdi and Sarmiento and identifies the specific sources of the theories and principles that inspired them.

Alberdi and Sarmiento, Botana reminds us, borrowed political ideas from societies with histories and environments different from their own. In planning for the future republican and democratic Argentina, the two men modified these concepts and made certain assumptions as they confronted

Argentine reality. Their arguments changed as new political writers appeared, but, as Botana convincingly demonstrates, their thinking was more consistent than the rhetoric or theory they employed would suggest.

Their underlying tenets can be summarized briefly. Both concluded that the colonial inheritance of Argentina was an inescapable obstacle to their plans and that only Northern European immigrants could introduce and develop republican institutions and customs, build flourishing cities, and expand the national economy. Both wanted for different reasons a strong central government. Alberdi argued that to further the formative work of the immigrants the government should concern itself solely with protecting their civil liberties. Political liberties would come with the new society they created. Left alone, without military obligations, the immigrants would increase trade with other nations and start modern industries. Schools would help them by teaching the applied sciences. Craftsmen and merchants gradually would replace soldiers in importance. Because the immigrants needed peace, Alberdi hoped to end civil wars and criollo politics by accepting the rural *caudillos* and the centralization of national power in Buenos Aires.

Sarmiento thought differently. His national government would plan and build communities of small, independent farmers and would control education because schools are the best creators of citizens. In his agrarian republic of equals the citizens, including naturalized foreigners, would forcefully exercise their political rights, and a professional army would protect the general interests of the state against subversion and revolutions. Until his republic became a reality, prominent criollos and propertied naturalized foreigners would govern.

Neither Alberdi nor Sarmiento anticipated what actually happened. Successful immigrants saw no need to become citizens. They concentrated instead on their personal affairs and insisted on their civil liberties against an expansive state. The criollos alone controlled the government, and they inaugurated military conscription. The immigrants who came and built Argentina had Mediterranean customs and habits rather than Northern European ones, and they were assimilated. Alberdi had his republic and his new world, Botana concludes, but Sarmiento did not.

Botana's study is well researched and well written. It should be read by those interested in the nineteenth-century origins of modern Argentina.

JOSEPH T. CRISCENTI
Boston College

FRITZ L. HOFFMANN and OLGA MINGO HOFFMANN.
Sovereignty in Dispute: The Falklands/Malvinas,

1493-1982. (Westview Special Studies on Latin America and the Caribbean.) Boulder, Colo.: Westview. 1984. Pp. xiv, 194. \$20.00.

Fritz L. Hoffmann and Olga Mingo Hoffmann have collaborated on a brief and informative history of the dispute between the United Kingdom and Argentina over the Falklands, or Malvinas, Islands, up to the Anglo-Argentine war of 1982. In twelve chapters the authors present the background to the confrontation and analyze the situation following the South Atlantic war. The Hoffmanns trace the lengthy history of the conflict, including the role of the United States and the consequences for inter-American relations. Twenty-two illustrations (mostly reproductions of historical maps), chronology of the Falklands from 1502 to 1982, and a very brief select bibliography are interesting and useful.

An introductory chapter provides encyclopedic, descriptive information about the islands. Five subsequent chapters follow the course of the dispute from its beginnings to the mid-1960s. The authors consider the question of who discovered the islands and examine eighteenth-century European colonial rivalry over them and subsequent Argentine national claims, culminating in the British occupation of the Falklands by force in 1833. They also discuss Britain's increasing hold over the islands and the improvement of relations with Argentina into the twentieth century, Argentina's renewed and increased questioning of the British role in their lives, and the anticolonial theme stressed by Juan Perón.

The next five chapters are devoted to events in the two decades preceding the 1982 war. The authors follow the Falklands/Malvinas question in the United Nations and bilateral negotiations that grew out of UN Resolution 2065 (1965). They narrate the declining course of those negotiations through hopeful agreements and proposals, increased Argentine impatience, and the British government's abandonment of serious negotiations after Parliament rejected its lease-back proposal in late 1979. They detail events surrounding Argentina's invasion to reclaim the islands in 1982 and the consequent war ending with a humiliating Argentine defeat.

The book concludes with a disappointing "Epilogue: An Evaluation," in reality a polemic revealing the authors' exasperation. This "analysis" of the situation following the war, earlier posited as a principal purpose of the book, is shallow. Its contentious tone contrasts with the authors' otherwise scholarly approach. This reviewer sympathizes with the general case made for Argentina's historic claims to sovereignty, the lack of U.S. understanding, and British intransigence, but that case is not convincingly argued in this seven-page chapter.

The major problem with the book is its brevity.

With only about one hundred fifty full pages of text devoted to a large and complex subject, a certain superficiality is inevitable. Nevertheless, the authors maintain a sharp focus on the Falklands/Malvinas question and bring together a great deal of scattered

information. Although not a landmark, the book is recommended as an accurate and interesting narrative on the subject.

G. POPE ATKINS

United States Naval Academy

Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

TARIQ ALI, editor. *The Stalinist Legacy: Its Impact on Twentieth-Century World Politics*. (Pelican.) New York: Penguin. 1984. Pp. 551. \$7.95.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

In his article "Hypothesis of Middle-Class Formation" (*AHR*, 90 [1985]: 299-338), Professor Blumin notes that many American writers by the end of the nineteenth century had not defined the term "middle class(es)" although they had used it. Blumin also uses the term without defining it—unless his list of "aspects of personal and social life most critical to an experiential hypothesis of middle-class formation" constitutes a definition. Let us assume so.

The proposed list includes "work, consumption, residential location, formal and informal association, and family organization and strategy." A few pages later the author suggests that "others may be added to the list."

Blumin's criteria vary somewhat from those submitted by other social scientists—usually sociologists. In any case there are three or more "aspects," "cases of social experience," features, components or criteria which the student and social analyst must utilize or keep in mind when using the term "middle class(es)." That is not merely difficult; I submit that it is obfuscatory, subjective, vague and self-defeating in its eclecticism.

Professor Blumin forewarned that, in applying the test, the five (or more) aspects "must coincide to such a degree that each type of experience reinforces the distinctiveness of each stratum." Thus he is able to throw into the same class "parents who taught their children to be thrifty and industrious (how will that be ascertained?) and who sent them to

high school in preparation for careers in business or business employment" (that would unite such parents with clerks along with others in "business employment" plus their owners) and white collar workers and entrepreneurs and a number of other vocations and people with quite different relations to means of production and circulation.

It does not make sense—historical, economic, political, social, or logical—to include in the same class employers and their employees. If an individual conformed to three or four, of the five criteria but not to the other one or two (or more), what then determines his or her class? If members of an ethnic minority fit four of the "aspects" but are denied entry into a community formal or informal association (fraternal society, golf club, church group, etc.)—to which class do they belong? Consider a black professional who conforms to all but one of the criteria; suppose he lives in a black ghetto, on what basis would Blumin disregard that example of social decadence in order to include him in the middle class?

Suppose one chooses to add size of income to the list, a criterion often included by many social scientists. How does one add or determine the degree of coincidence involved by considering size of income, kind of work, occupation, vocation, residential location, etc.? What weight, and how is that determined, would be given to each component?

While Blumin apparently was limiting his analysis to the nineteenth century, do not the same problems nag the analysts of this century? For example, some say that skilled wage workers—blue collar or otherwise—belong to the middle class. Other middle class members will have none of that so that conforming to two of Blumin's "aspects" is impossible. Blumin considers white-collar workers as members of the middle class, ignoring all his other criteria and displays a distasteful bias in referring to moving from white collar status to manual working class as "slipping down." Discrimination based on color, religion, sex, ethnicity, etc., affect the objective determination of class membership when the criteria

are multiple. I suggest that Occam's razor is applicable. Need the criteria be multiple?

As opposed to Blumin's proposal I submit the following: All categories of wage workers constitute a class. The feature of earning a living by being paid wages provides a homogeneous character despite the many differences arising from income size, nature of work performed, the work site, religion, residential location, and so on. The definition is valid not only for this century but also for, at least, the last decades of the nineteenth century.

All extremely independently wealthy people and owners of large enterprises individually employing many scores of wage workers constitute the upper class(es).

All those who are intermediate—such as self-employed, proprietors of small and medium size enterprises—comprise the middle class(es).

Vicissitudes and differences in the character of work, class consciousness or lack of it, class awareness or lack of it, self-serving delusions, values subscribed to, family strategy, nature of residence and its location, voluntary associations, community organizations, size of income, religion, formal education and where obtained, and more "aspects" are involved indeed in analyzing or in comprehending societal phenomena but none is pertinent to defining a socio-economic class.

ALBERT PRAGO
Flushing, New York

PROFESSOR BLUMIN REPLIES:

Mr. Prago seriously misrepresents my argument, and seems to miss as well the purpose and the spirit of my article. At no point did I claim that the five areas of social life that I discussed in conjunction with the issue of middle-class formation constitute a definition of "middle class." Nor did I offer any other definition of that term. Rather, I attempted to find practical ways of examining the interesting and complex question of middle-class formation and the larger question of the salience of class (or if one prefers, hierarchical forms of social differentiation) in nineteenth-century America. All too often the pursuit of answers to questions like these become bogged down and even trivialized by cookbook definitions of social class, Marxist or otherwise, and I would point to Mr. Prago's own recipe as a case in point. Must clerical employees be considered as having been in a separate class from their employers? Should we simply dismiss the very large number of nineteenth-century documents that suggest in a variety of ways that clerks were members (or members-in-training) of a business class that excluded wage-earning workers and most small master mechanics? Is it not wiser to examine questions

such as these with as few encumbrances as possible from definitions that presuppose the bases for social differentiation in past societies?

I am not trying to suggest that scholars neglect to define their terms, and I believe I have defined mine clearly enough. But what I defined were the terms of my inquiry, not its final product: "Did significant aspects of the daily lives of middling folk change during the nineteenth century in ways that indicate the formation of a new, relatively coherent, and relatively clearly bounded (if not self-conscious) social group, located in an intermediate position within a hierarchically organized social structure?" (p. 312). To assume that we already know the answer to this question, or to subordinate it to some larger and fixed theory of social differentiation, is to preclude inquiry, not to direct it into paths of fresh discovery.

STUART M. BLUMIN
Cornell University

REVIEWS OF BOOKS

TO THE EDITOR:

Three things should be expected of even the most unassuming review: that it characterizes the approach of the book to be reviewed, that it points out the basic chain of reasoning, and that it shortly describes the results. On this basis criticism may be advanced. If the review is more ambitious, it will speak about the author's method, characterize the source materials used by him, and fix the book's place within the existing literature. I am afraid it must be said that the reviewer of my book, *Marxismus und Industrielle Revolution*, did not meet even one of these requirements (*AHR*, 89 [1984]: 1041-43).

One thing though does the review prove without any shade of doubt: that the reviewer read this book and the two earlier books of the trilogy with intense displeasure. How should he not, since right from the start he has Heinrich von Treitschke before his eyes! But to begin with he gives his opinion about the style, which, it is true, reminds him of Thomas Mann, but which is said to be "simply fatiguing" in a historical book. Therefore even the mere size of the trilogy is offensive to him: 2000 pages. The shortcomings of the style, according to the reviewer, are obviously due to the author's claim of "thinking deeply about topics of large import and wide concern," and that by disregarding the "increasing specialization" in the historical profession. On the other hand, the reviewer finds a "thicket of footnotes" on every page, and the "Rankean" virtues of

erudition are obviously apparent, only, "in a smothering procession of names, dates and terms." Another reviewer would perhaps have indicated that the number of pages has to be seen in relation to the subject and to the time spent on the books (2000 pages about the "modern ideologies and their practical realization" in 20 years are, as concerns the quantity, certainly a rather modest achievement), and he might have added that the only adequate, although difficult method, of a scholarly treatment of this subject is to establish a close connection between the basic thought and the significant details.

Concerning the set-up of the book the reviewer just mentions that it consists of two parts of which only the second one is said to answer the title. The "rich and stimulating literature dealing with industrialization and Marxism" has allegedly not been taken notice of. By this he presumably means the literature which either deals with industrialization or with Marxism. This is indeed much too voluminous as to be completely known to and used by one single author. Or should the reviewer be able to name many titles for which the "and," and the "and" alone, is the subject? Even Eric Hobsbawm, reviewing the book in the *Bulletin* of the German Historical Institute in London only mentions George Lichtheim, whereas Richard Löwenthal is of the opinion (*Historische Zeitschrift*, 240) that up to now nobody has made Marxism and Industrial Revolution the topic of a book, no matter how astonishing this fact must appear.

The reviewer nevertheless also finds approving words. Not a few books have been written about Marx's "anti-Semitism," but, in the reviewer's opinion, the author holds a well-balanced and plausible middle position between the extremes. Now, the section referred to comprises four pages (pp. 479–82), and it is only part of a sub-chapter. Thus, well acquainted with the sources and the secondary literature, the author must have written a very concentrated and precise piece of prose, whereas on the remaining 652 pages he expressed himself in a longwinded and tiring way. Indeed, the reviewer seems to take his readers as very gullible.

Should the key to the riddle be found in the statement that the author's leanings (as those of Treitschke) are "ostensibly reactionary and ultranationalistic"? But what is the reviewer's idea of "reaction," if the author is able to do Marx justice (in a question so exposed to hostile comments), and what can "ultranationalistic" mean in a book in which hardly a word is said about Germany?

The reviewer maintains that I need a censor. I would indeed welcome a censor who advances arguments and offers documentary evidence. In the *AHR* I have unfortunately only found a reviewer

who makes allegations and reveals opaque resentments.

ERNST NOLTE
Freie Universität Berlin

PROFESSOR LINDEMANN REPLIES:

I regret having so offended Ernst Nolte. Contrary to his suspicions, I bear him no malice, and I harbor no opaque resentments against his political beliefs or his nation. He is even wrong to seize upon my mention of von Treitschke as somehow revealing my true feelings. My review merely remarked that Nolte's writings tend to evoke strong feelings, for and against, and that some observers associate him with "an older and now suspect generation of historians." I personally am inclined to consider von Treitschke, for all his obvious faults, an often unfairly, or at least facetiously, maligned historian. I have, at any rate, long made it a special point, in my own teaching and writing, to avoid simplistically negative judgments of German history, German national traits, or German conservatism.

As I quite explicitly stated, I found much to admire in Nolte's work, above all in its erudition, ambition, and scope. I learned from the book under review. Much of it, however, I found crabbed and tiresome. I will admit to the "prejudice" that German scholarship at times tends in the direction of learned obscurity and of conspicuous rather than truly useful detail. The main edge of my criticism, similarly, reflected my regret that Nolte has not presented his ideas more clearly and economically. I strived to write a review that avoided polemics and ideological innuendoes, although it seemed to me appropriate to take note of both the controversy and admiration surrounding Nolte's work.

Having made these points, I must express objections concerning both the tone and content of Professor Nolte's letter—even granting that letters of protest over critical reviews do not usually set high standards of judiciousness. I make no claim to having composed a flawless review, and as I remarked in the review itself "a short review of a work so vast and often elusive can easily give a misleading impression by concentrating on a single aspect or debatable point." I chose the passages dealing with Marx's anti-Semitism as an "appropriate example," certainly not the only possible example. I nowhere asserted or even vaguely implied that "the shortcomings" of Nolte's style "are obviously due to . . . [his] claim of 'thinking deeply.'" It is true that I did not "point out the basic chain of reasoning" of Nolte's work, but that is simply because I could not detect anything that resembled a chain. The image I would choose is that of tangled coils whose points of

connection, if they exist, are less than obvious—and nearly impossible to describe in a short review.

I did not assert that ultranationalistic remarks are to be found in the book under review. I said: "Nolte's penchant, in his previous works [n.b.], for piquant obiter dicta and ambiguous speculation, often self-indulgent or ostensibly reactionary and ultranationalistic, have on occasion drawn attention away from his more serious thought and scholarship, or inclined reviewers to be more severe in their judgments than they might otherwise have been (misfortunes for which Nolte has only himself to blame, it seems to me)." An appropriate example of the kind of remark that others have objected to is Nolte's statement in his *Deutschland und der kalte Krieg* that American actions in Vietnam represented "after all nothing less than [a] more cruel version of Auschwitz."

I hope that readers of the Communications section who might be tempted to give credence to Professor Nolte's complaints will take the time to look back at the original review to judge for themselves. They might want to look as well at the remarkably similar objections he posed to Felix Gilbert's review of the above-mentioned volume (*AHR*, [1977]: 235–36), although Professor Gilbert, in a longer review than mine, did take on the heavy task of presenting the chain of reasoning of that book—and was scolded by Nolte for "contradictoriness," carelessness, and various other forms of obtuseness.

Responding to Professor Nolte's complaints in the confines of 700 words is an exercise in frustration, but I suggest that if he is so often misunderstood and misinterpreted, the fault lies primarily in his own inability to present his ideas intelligibly and with sensible regard for his readers, and not to the ill will and incompetence of his reviewers.

ALBERT S. LINDEMANN
University of California,
Santa Barbara

TO THE EDITOR:

Robert Bremner's review of my book, *Building Character in the American Boy: The Boy Scouts, YMCA, and Their Forerunners, 1870–1920* (*AHR*, 89 [1984]: 1169–70), tempts me to respond, both because it differs markedly from other reviews of the book and because it illustrates some reviewing practices that are deeply annoying to authors and unhelpful to readers of reviews.

An old favorite is the list of overlooked sources, usually placed near the end of the review. After praising the range of my research, Professor Bremner more or less retracts the praise by citing four items he failed to find in my notes. Two (books

by Jane Addams and Charles Loring Brace) are actually cited in my book, on pages 319 and 327 respectively. A third (an amusing but not entirely convincing portion of H. L. Mencken's autobiography) got left on the cutting room floor as I heeded my editor's request to cut "see also" references. And the fourth (Paul Fussell's review praising the 1979 Boy Scout handbook) simply did not seem vital. In any case, who cares? Unless a reviewer can argue that omitted sources would have materially altered the book's conclusions, a list of oversights is of interest to nobody except the author whose thoroughness has been impugned. This sort of niggling criticism is petty and—as Bremner demonstrates—prone to error.

Quarreling with the title is another reviewing foible. In the spirit of Ralph Nader, the reviewer reveals that behind a broad title hides a mere monograph—as if readers had no inkling that a title commonly states a broad theme, while the subtitle explains the book's scope. Professor Bremner adds a warning that most of my book is confined to the last decade or two indicated in the subtitle. In fact, there is much on social and cultural changes before 1900, and there are three short chapters and other passages directly on nineteenth-century boys' work. Is this unreasonable in a book tracing the origins of a phenomenon that began well back in the nineteenth century but culminated on a large scale only after 1900?

Having cut the book down to size, Bremner then announces condescendingly: "The narrow focus of the study does not preclude David I. Macleod from passing judgment on a variety of subjects—social control, the middle class, Progressivism, juvenile courts, and philanthropy—on which he seems to have strong opinions." This sort of criticism by innuendo is the worst fault of all, enabling a reviewer to imply a multitude of faults without having to specify any.

In fact, what I did was perfectly reasonable. I sought to place my subject firmly in its social and cultural context and to suggest implications of my work for related subjects of established interest to historians. I also tried to write vividly. Unless he or she merely wishes to write antiquarian trivia, a social historian should do no less. And a critic should do more than sneer.

In a milder vein, Professor Bremner announces that historians of philanthropy "will find much to ponder, profit from, and, no doubt, dispute" in my work. Admittedly, a reviewer pressed for space cannot do everything, but once again the effect is dismissive. Individually trivial, such remarks combine with other quibbles to create a cumulatively slighting tone that soon pervades the review, denigrating the book in question without requiring the effort of substantive criticism.

As for the reader of the review, he or she can only guess the underlying reasons for the reviewer's displeasure. Even I can only guess. Perhaps Professor Bremner objects to my moderately critical tone in assessing the efforts of would-be philanthropists and my insistence that Boy Scout and YMCA youth work—whether regarded as social reform or as social control—was an endeavor not only *of* and *by* but also *for* the middle class. In stressing the importance of programs other than those directed towards the lower classes, I believe my work in a small way redresses an imbalance in historical writing on youth, philanthropy, and the Progressive Era. My emphases do indeed differ from those in Professor Bremner's work. And my contentions are certainly debatable. But if they are the problem, they should be openly debated—even in a brief review.

DAVID MACLEOD
Central Michigan University

PROFESSOR BREMNER REPLIES:

In the space allotted me I indicated my appraisal of the scope, tone, limitations, and achievement of David Macleod's book. I am sorry if my estimate of its value does not match the author's.

I mentioned the "overlooked sources" because I thought they would have enlivened and enlightened the book and were worth the attention of both the author and readers. To say that scholars may find much to dispute as well as much to ponder and profit from in the book does not denigrate a work whose contentions, the author states, are debatable. If my review "cut the book to size" it was because I thought that, despite its merits, it was inflated by self-importance. I sought to convey my feeling about this aspect of the book in a way that would not embarrass the author.

ROBERT H. BREMNER
Ohio State University

TO THE EDITOR:

In his review of Justin McCarthy's *Muslims and Minorities: The Population of Ottoman Anatolia and the End of the Empire* (AHR, 90 [1985]: 191–92), Mark Pinson gratuitously criticizes the preparation of Ottoman historians in recent years for failing to provide them with "training in the languages or history of any of the subject peoples." The statement is incorrect, as is evidenced by Dr. Pinson himself, who because of his interest in the Slavic subjects of Southeastern Europe obtained the necessary training in their languages and history as well as Ottoman Turkish while working for his Ph.D. at Harvard University. All Ottoman historians are

trained in the languages and histories of the two largest groups of subject peoples, namely the Turks and the Arabs. There would be no Ottoman historians if, in addition, they were required to learn Greek, Hungarian, Armenian, Bulgarian, Serbian, Rumanian, and numerous other languages before receiving their degrees but, evidently, those interested in particular minority groups must learn their languages before researching their history. The trouble in Ottoman studies comes not from Ottoman historians lacking requisite linguistic training beyond Turkish and Arabic but, rather, from the historians of the non-Turkish subject peoples, including the Arabs, who fail to use Turkish sources in studying those people under Ottoman rule, resulting in serious distortions and prejudice. There are, moreover, all too many diplomatic historians who while commendably studying all relevant documents in European and American archives regarding European diplomacy concerning the late Ottoman empire, entirely ignore the voluminous Ottoman diplomatic documents found in the archives of the Turkish Ministry of Foreign Affairs as well as in the Prime Minister's Archives, both in Istanbul, and instead content themselves with accepting what European diplomats assumed the Ottomans' attitudes, positions, and policies to be.

McCarthy is to be commended for examining all available statistical materials concerning the population of the Ottoman Empire and subjecting them to rigorous analysis on the basis of demographic techniques. That none were available with the range and depth of the Ottoman census reports was certainly not his fault but, rather, that of *millet* leaders who preferred to limit the tax and conscription obligations of their followers by using invariably inaccurate guesses instead of more accurate counts which could have been made on the basis of their own baptismal and other records. On the tendency of non-Muslim religious leaders during and after World War I to falsify their own population statements for political purposes, see McCarthy's "Greek Statistics on Ottoman Greek Population," *International Journal of Turkish Studies*, I (1980), 66–76.

Dr. Pinson also tends to reflect the longstanding belief in Europe and America, which McCarthy sought to dispel in this study, that the Ottomans made no census counts, or that, if they did, their figures were inaccurate. In refutation of this he might well have consulted in addition my own studies, "The Ottoman Census System and Population, 1831–1914," *International Journal of Middle East Studies*, VI (1975), 94–114, and "Ottoman Population Movements During the Last Years of the Empire, 1885–1914," *Journal of Ottoman Studies*, I (1980), 191–205, and Kemal H. Karpat, "Ottoman Population Records and the Census of 1881/82–1893," *IJMES*, IX (1978), 237–74. The Ottoman

statistics, while certainly subject to careful analysis as McCarthy shows, were no more inaccurate than similar census efforts undertaken in Great Britain and Western Europe at the same time. The particular value of McCarthy's book is that he has recognized these deficiencies and reconstructed the Ottoman figures to provide what will certainly be the definitive study of the population of the last century of Ottoman existence.

STANFORD J. SHAW
University of California,
Los Angeles

DR. PINSON REPLIES:

Several of Professor Shaw's assertions appear questionable to me. First of all with respect to Ottomanists having studied Arabic as well as Turkish, I have a strong impression that, while most of my colleagues of my cohort (that of the 60s, the largest) have studied Arabic, they did so for a short period, one or two years, just to know enough to enhance substantially their understanding of Ottoman. Arabic is not Spanish and this amount of study does not produce easy, fluent reading. Accordingly, while most of us have some Arabic, we do not use it anywhere near as frequently as we do other languages of the Ottoman Empire. I readily admit to having no statistical data on this question, but neither does anyone else.

Secondly, Professor Shaw is on very thin ice casting me as a *Gegenbeispiel* to my own contention. My studies to prepare me to deal with non-Turkish peoples of the empire, including not only the Slavs, whom Shaw mentioned, but also the Greeks, Jews, and Romanians, were far from a normal part of the curriculum. All of it was done on my own initiative, in some cases against "official" opposition, and a substantial portion entailed studying at other institutions subjects not offered at Harvard.

Finally, Professor Shaw is worried that broader training in the peoples of the empire, such as now appears to be more common in the study of the Habsburg Empire—a very positive trend—would mean "there would never be any Ottoman historians." What is his rush to train new Ottomanists? Like a number of my fellow Ottomanists and Balkanists whom I know, I have been forced, by the absence of academic teaching positions in my field (in my case over a five year period), to retrain for an entirely different profession, in my case, business librarian. I assume that the half a dozen or so others I know in my position, and the many others I do not know, would all ask Professor Shaw, since qualified specialists far outnumber the available positions, why there is any need to train new specialists. Is

there any need for perhaps a decade to train any more?

MARK PINSON
Cambridge, Massachusetts

TO THE EDITOR:

I have no doubt that Robert J. McMahon read my book, *Anglo-American Relations and Colonialism in East Asia, 1941–1945* (*AHR*, 90 [1985]: 239–40). I will say, however, that his review was so superficial that it could have been written by someone who did little more than thumb through the Table of Contents, Bibliography, and Appendixes.

Professor McMahon dismisses my book principally because of the existence of the excellent books by William Roger Louis and Christopher Thorne. On one hand, McMahon describes my research as "prodigious"; on the other hand, he criticizes me for not taking into account the interpretations of Louis and Thorne. Yet I took the extraordinary steps of acknowledging my intellectual debt to Louis and Thorne and delineating some of our differences. Surely McMahon does not mean to suggest that Louis and Thorne have preempted the general subject of colonialism and the war against Japan.

McMahon acknowledges, but curiously disregards, one feature of my work in "supplying some fresh quotes and . . . new documents." I would have thought that unearthing new material represents a basic objective for historians.

He cavalierly characterizes my analyses of certain topics (e.g., Atlantic Charter, trusteeship, Indochina, and postwar planning) as lacking originality. These topics, however, are fundamental to any examination of the general subject. Indeed, one need hardly wonder about any responsible reviewer's reaction if these basic topics had been omitted. Moreover, McMahon fails to demonstrate, beyond his simple assertion, that my treatment of these topics merely echoes the interpretations of Louis and Thorne. Even historians of the magnitude of Louis and Thorne could not cover everything! Some of my contributions to these basic topics include examination of such issues as the connection between Chiang Kai-shek's 1942 visit to India and subsequent British and American studies of the future of dependent peoples, Soviet-American interplay at the San Francisco Conference concerning trusteeship, Roosevelt's anti-colonialism, Roosevelt's "non-policy of determined drift," and Churchill's restrictions on the Atlantic Charter.

I find it odd that McMahon criticizes my book as unoriginal and then dismisses as "idiosyncratic" and "marginally related" my introduction of new dimensions to the general subject. The topics which he shuns—such as the investigation of the attack at

Pearl Harbor (Churchill announced he had "worked for" the U.S. entry), the Anglo-American debate over military strategy (to recover British colonies?), and the unexpected consequences for Western colonies in Asia of the use of the atomic bomb—seem appropriate to any detailed examination of colonialism and the war against Japan.

Through the use of selective quotations, McMahon not only rejects my interpretive judgments but also makes me sound like a pie-eyed innocent. His criticism centers on my alleged neglect of Roosevelt's realism in maintaining harmony with his Western allies as opposed to my hand-wringing lament that Roosevelt failed to secure "idealistic and reasonable" objectives. In fact, I presented—and continue to hold—a balanced view of the matter. Most of Chapter VII, for instance, is devoted to "four major reasons [which] explain the weakening of American objectives" (p. 154). These are: the absence of widespread international support, the desire of the Joint Chiefs of Staff to control strategic points in the Pacific, and—yes—the need to cooperate with the Western allies not only (a) during the war but also (b) after the war.

On the other side of the ledger, I would have thought that the bloodshed and turmoil in Asia over the past forty years (as a result of nationalist-imperialist struggles) is generally recognized as a "tragedy." Let me hasten to add that I do not take the view that the events in 1941–45, or the actions of any war leader, "caused" this "tragedy." I do regret, however, that during the war the problems associated with emerging nationalism in Asia could not have been addressed more effectively. At Yalta, the Big Three leaders announced they held "the greatest opportunity in all history to create in the years to come the essential conditions" for a lasting peace. In writing that portion of my book which presents this one side of what surely is a many-sided, complex argument, I have simply taken them at their word. I offer alternative views as well.

JOHN J. SBREGA
Tidewater Community College

TO THE EDITOR:

I wish to thank you for publishing a review of *Martin Luther: The Man and the Image* (AHR, 90 [1985]: 435–36) and to comment on certain features of the review. Professor Minnich's opening paragraphs reveal that he has missed the point of the book. This is not surprising, for like most academics he is a stranger to modern psychiatry, and the book is a psychiatric study of Luther's personality and career. It is impossible to portray Luther correctly without utilizing this discipline. Luther himself reveals that he suffered depression from his adolescent years

onward, he developed manic symptoms by 1520, and in later years became markedly paranoid. Minnich erroneously refers to my book as a psychoanalytic study, unaware that psychoanalysis is an attempt to account for neurosis and is rather a sort of ideology than a medical discipline. It is true that Luther went through a neurotic stage, but for most of his adult years he was psychotic, that is, cut off from the real world in certain areas.

Psychiatric study of Luther was initiated by the Copenhagen psychiatrist Dr. Paul J. Reiter. This pioneering work was based on the psychiatry of the 1920s; subsequent advances in the understanding of psychotic disorders now make it possible to provide a much fuller explanation of Luther's behavior than Reiter gives. Anyone who wishes to see how psychiatry has advanced in the present century may consult the successive editions of a medical textbook called *Modern Clinical Psychiatry*. The first edition (1934) was prepared by Dr. A. P. Noyes; in the 1950s he was joined by Dr. L. C. Kolb; the tenth edition appeared in 1982.

To illustrate how Luther's psychosis affected his utterances, we may consider the prefaces to the German version of the NT he published in 1522. He states that John's gospel contains many of the "words of Christ" but few of his works, while the other gospels give us many of his works but few of his words. Since the works "are of no help to me," only John's gospel is worth one's attention. Actually Matthew has twice as many of the "words of Christ" as John. This amazing falsification indicates that Matthew has material that Luther could not stomach, and he here resorts to the defense mechanism called "denial" to defend against the pathological anxiety aroused in him by Matthew. In Luther's complete works there are thousands of comparable falsifications. Luther is not to blame for such behavior, for psychosis is an inherited disorder and in his day no professional therapy was available.

This is how Luther appears to an objective scholar unacquainted with psychiatry. Willibald Pirckheimer (1470–1530), the learned humanist of Nuremberg, after reading many of Luther's tracts of the 1520s, wrote: "In earlier years almost all men applauded the name of Luther, but nowadays nearly all are overcome with disgust upon hearing this name. . . . In addition to his insults and his audacity, impudence, arrogance and abusive language, he puts on such a bold front he will stop at no lie." How do present day Luther cultists—the academics championed by Minnich—handle the problem? They shamelessly suppress a mass of unpalatable data, fabricate an image of Luther to their taste, and attack anyone who exposes this brand of scholarship. As for the soundness of my scholarship, which Minnich impugns, my Princeton dissertation—first published in 1940—has proved so helpful to gradu-

ate students in sixteenth century literature that it is still in print, listed by three reprint houses. Regarding Minnich's objection to the tone of my book, I happened to come under the malign influence of Erasmus at an early age, and the spectacle of Luther's grandiose delusions being interpreted almost as a divine revelation led me to write in the spirit of the *Julius Exklusius*.

HERBERT DAVID RIX
Old Saybrook, Connecticut

TO THE EDITOR:

I have yet to read Shmuel Krakowski's *The War of the Doomed: Jewish Armed Resistance in Poland, 1942-1944*. I must, however, take historical issue with Professor Stephan Horak's review of the Krakowski book (*AHR*, 90 [1985]: 456-57). The reviewer writes that "the Jewish partisan movement predated Polish underground activities." This statement is simply historically untrue and it can be verified as such by any beginning student of Eastern Europe.

The Polish underground was organized in September-October 1939. The implementation of the underground followed orders from Marshall Rydz-Smigly who sent a special courier plane from Rumania to beleaguered Warsaw on September 26, 1939. General Michal Karaszewicz-Tokarzewski became the first leader of this organization called "Służba Zwyciestwa Polski" (SZP) (In the Service of Poland's Victory). Following several name and organizational changes this movement became "Armia Krajowa" (AK) (Home Army) on 14 February 1942.

It must also be pointed out to the reviewer that the Polish 1939 army continued to fight as partisan units, the most famous being led by Major Henryk (Hubal) Dobrzanski who was killed in battle in May 1940. This Polish armed resistance to the Germans never ceased; to write otherwise, unfortunately, is historical misinformation.

RICHARD WOYTAK
Chapman College

PROFESSOR HORAK REPLIES:

Professor Richard Woytak could have saved his letter to the editor had he read first Shmuel Krakowski's book. On page 296 he could read the following: "Most of the Jewish partisan units were established during the second half of 1942, when the Polish partisan movement did not in effect exist." Perhaps it was my oversight having not used quotation marks, yet it should have been obvious to him that I got this information from Krakowski's book. Therefore, I consider his lecturing me in

Polish history to be unwarranted and out of place. As to Professor Woytak's information on the Polish underground, I personally am aware of that, yet it can be argued about who began first to populate the forests since activities of Major Dobrzanski can also be considered as a continuity of the 1939 warfare.

STEPHAN M. HORAK
Eastern Illinois University

TO THE EDITOR:

I wish to protest in the strongest possible terms the obvious breach of professional ethics involved in the choice of Professor Gerald D. Feldman as reviewer of Henry Turner's *German Big Business and the Rise of Hitler* (*AHR*, 90 [1985]: 717-18). The close relationship between Professors Feldman and Turner is well documented. Your review editor must have known that this assignment would involve the appearance of a conflict of interest, if not the actuality of such a conflict. Surely it is the policy of the *Review* to avoid such a situation whenever possible. Furthermore, the *Review* has allowed itself to be used by Professor Feldman as a vehicle for his polemic against Professor David Abraham. That too is intolerable.

The fairness and objectivity of the *Review* has been called into question. I believe the profession deserves an explanation of this departure from normal practice. I look forward to hearing from you about this important matter.

JOHN R. GILLIS
Rutgers University

THE EDITOR REPLIES:

AHR reviewers are chosen by the editor and associate editor from lists of qualified scholars drawn up by the staff. I selected Gerald Feldman to review the book by Henry Turner for the same reason that the Princeton University Press chose him to evaluate David Abraham's manuscript before its acceptance for publication. His research and publications have made him the foremost authority on the relationship between German business and politics in the imperial and Weimar periods. In the past he has, furthermore, often differed in print with aspects of Turner's approach to the subject. That he happens now to agree with Turner's views on standards of scholarship is no disqualification.

Gillis's intemperate letter shows the depth of the division within the profession on the Turner-Feldman-Abraham affair. Passion has outrun reason, but then passion is always fleeter. Book review editors find it difficult to select qualified reviewers for Turner's book who are not aligned with one

camp or the other, and few of those chosen will want to avoid referring to the general controversy in their reviews. Witness the review published by the *New York Times* on January 27, 1985.

A look backward ought to convince any reasonable person that the *Review* has been even-handed in its handling of the disputants. The reviewer chosen for David Abraham's book happened to be ideologically sympathetic to his point of view, although that played no role in the choice. I gave Abraham all the space he desired in which to respond to the letters of Tim Mason and Henry Turner in the first public airing of the controversy (*AHR*, 88 [1983]: 1142-49). That I did not permit Turner to react to Abraham's response in the same or a later issue led Turner and Feldman to publish a protest in *Perspectives* (May-June, 1985), calling for a change in the *Review's* policy prohibiting more than one exchange of opinions in our communications section. Abraham has twice reviewed for the *AHR* and on one of those occasions was allowed to cite Turner in a manner protested by Feldman in a recent issue of *Central European History*.

In brief, the books by Abraham and Turner have now been reviewed sympathetically in the pages of the *Review*. Both reviews have been attacked in our communications section. To be criticized by both sides in the dispute, in which there appear to be few, if any, neutrals, is the best measure of the *Review's* impartiality.

OTTO PFLANZE
Editor

ERRATA

TO THE EDITOR:

I have noticed that my review of Harry E. Dembkowski's monograph, *The Union of Lublin: Polish Federalism in the Golden Age* (*AHR*, 89 [1984]: 483-84), contains an error. Its opening paragraph begins as follows: "On July 1, 1659 . . ." The correct date, however, should be July 1, 1569. I do hope that the author and the readers will accept my sincere apology. Mea culpa!

A. B. PERNAL
Brandon University

TO THE EDITOR:

In re Charles Tilly's review of Barbara Tuchman's book, *The March of Folly* (which I have not read) (*AHR*, 90 [1985]: 386-87), surely the words of that great sociologist and historian of the family, W. S. Gilbert, are appropriate, in slightly modified form:

Chorus. I wouldn't say a word that could be
reckoned as injurious,
But to find a father (George III, born 1738)
younger than his son (Lord North, born 1732)
is very curious.
And that's the kind of father
that is usually spurious,
Taradiddle, taradiddle, tol-lol-lay.

Iolanthe

MAURICE LEE, JR.
Rutgers University

PROFESSOR TILLY REPLIES:

Buttercup. Things are seldom what they seem.
Skim milk masquerades as cream;
George the Third looks like his father,
Prince of Wales—oh, what a bother!

Captain. Nonetheless,
Now confess.

Buttercup. Tuchman's *Folly* did not err,
Tilly made the slip, kind sir;
In haste, confusion, sloth ad lib,
Gave North for dad North's own half-sib.

Captain. Heed my roar:
Err no more.

H. M. S. Pinafore

CHARLES TILLY
New School for Social Research

THE EDITOR REPLIES:

Mikado. My object all sublime
I shall achieve in time—
To make the punishment fit the crime—
To make the punishment fit the crime—
And make each reviewer penitent
Unwillingly represent
A source of innocent merriment!
Of innocent merriment!

The Mikado

OTTO PFLANZE
Editor

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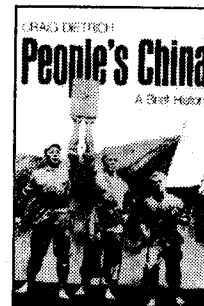
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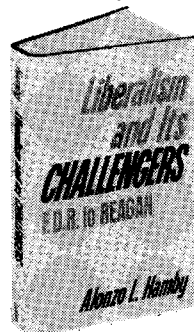
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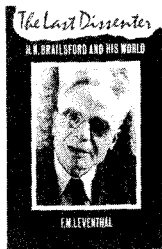
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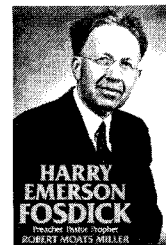
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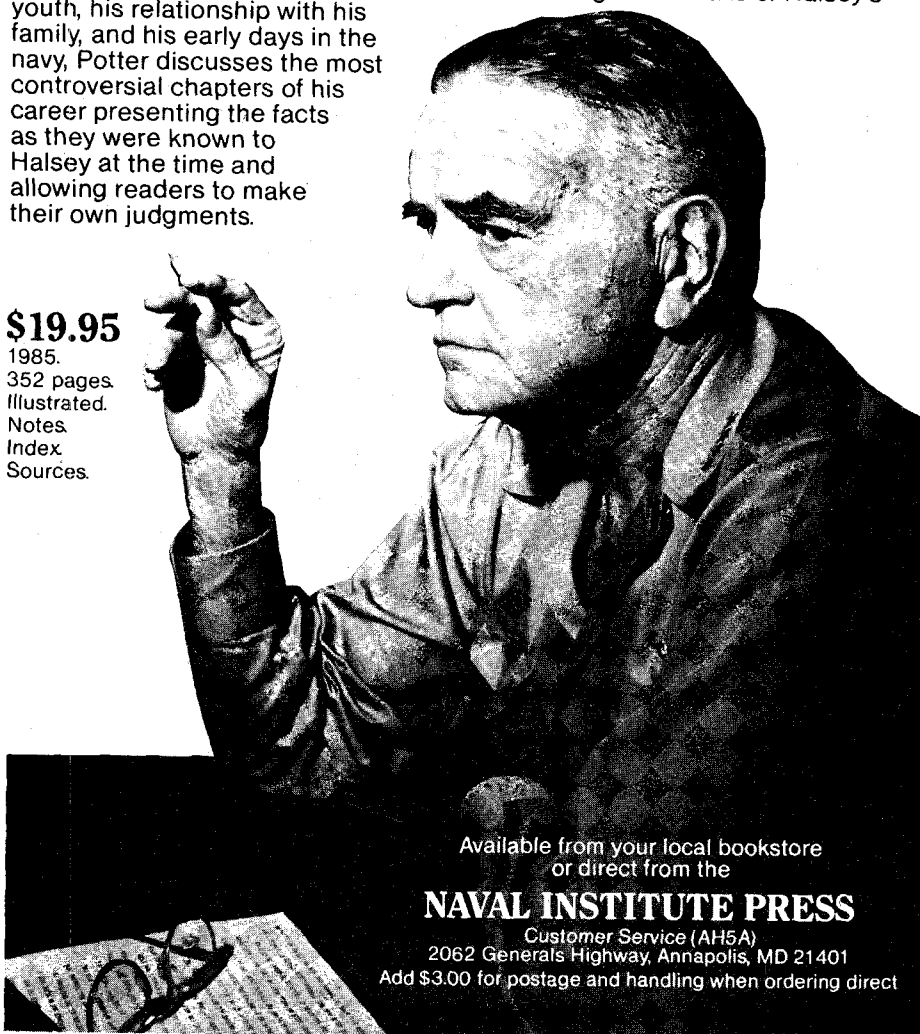
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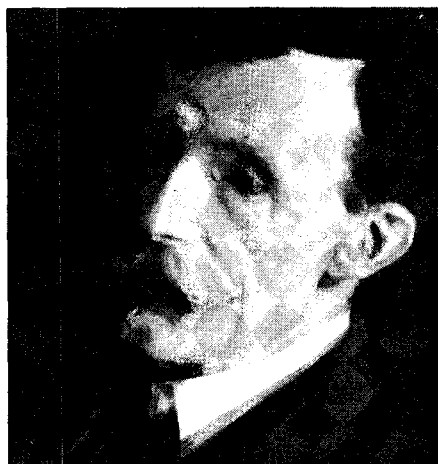
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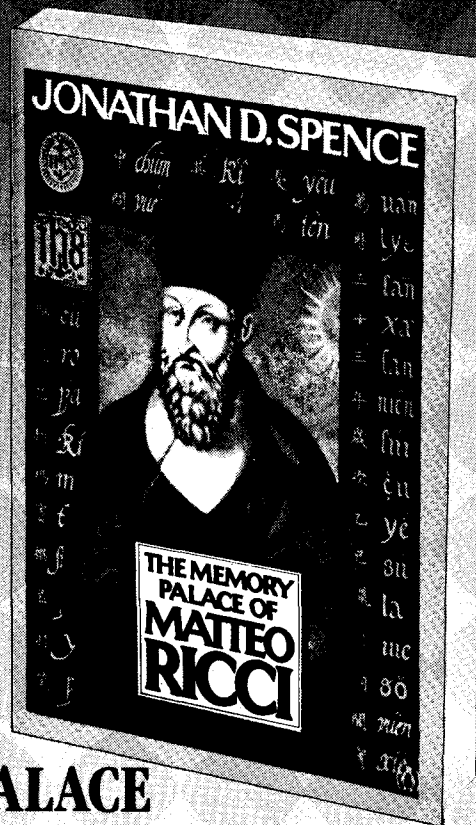
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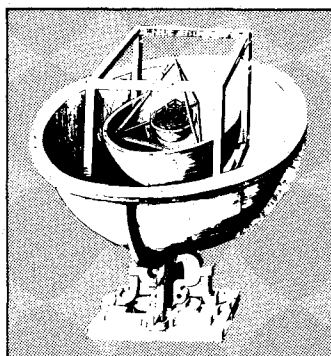
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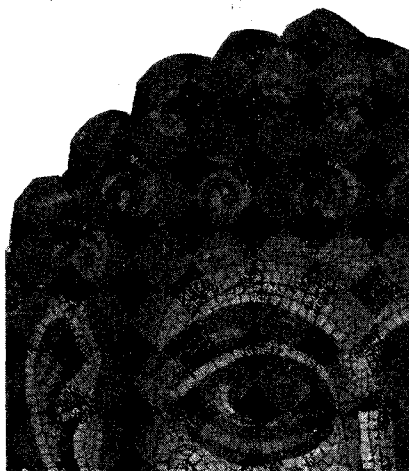
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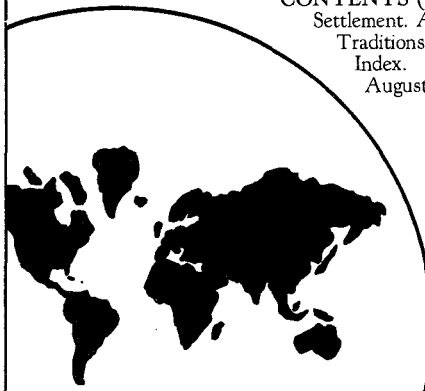
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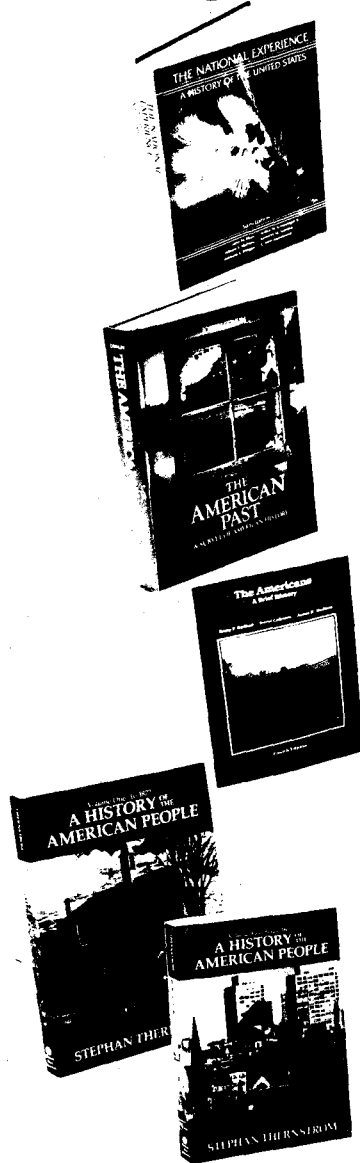
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